SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, JANUARY 13, 1905.

CONTENTS:

Academic Ideals: PROFESSOR R. S. WOODWARD 41

The American Association for the Advancement of Science:—
The Present State of Geodesy: O. H. TITTMANN 46
Future Developments in Physical Chemistry: PROFESSOR WILDER D. BANCROFT 50
Reports of Committees 59

Scientific Books:—
Granville on the Differential and Integral Calculus: PROFESSOR JAMES PIERPONT. *Venable on the Study of the Atom:* E. T. ALLEN 64

Societies and Academies:—
Section of Geology and Mineralogy of the New York Academy of Sciences: PROFESSOR JAMES F. KEMP, DR. E. O. HOVEY. *The Philosophical Society of Washington:* CHARLES K. WEAD. *Michigan Ornithological Club:* A. W. BLAIN, JR. 66

Discussion and Correspondence:—
Interesting and Important Facts: G. K. GILBERT. *Specialization, Ignorance and Some Supposed Palliatives:* DR. FRANCIS B. SUMNER. *Ultra-Violet Light in Photomicroscopy:* DR. CLIFFORD RICHARDSON. *How does Anopheles Bite?* PROFESSOR JOHN B. SMITH 68

Special Articles:—
Discussion in the British Parliament on the Metric Bill: WILLIAM H. SEAMAN 72

Current Notes on Meteorology:—
Temperatures in the Free Air; Bad Weather, Good Roads and Farmers; Monthly Weather Review; No Secular Change of Climate in Tripoli; Climatic Changes in the Lake Chad Region; Kite Meteorology over Lake Constance: PROFESSOR R. DEC. WARD 75

First Observations with 'Ballons-Sondes' in America 76

Scientific Notes and News 77

University and Educational News 80

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ACADEMIC IDEALS.*

THE beginning of an academic year calls up at once the lighter thoughts of pleasing associations and the graver thoughts of inspiring obligations. Here on the tableland of intellectual life youth and age meet to labor for a season in the fields of knowledge and discovery. The confident optimism of youth seeks to be chastened by the gentle admonition of experience. Youth imparts its buoyancy to age, age imparts its wisdom to youth, and both are kindled by the glow of elevating aspirations. It is a time, therefore, for a blending of our lighter and our graver reflections.

Being delegated for the moment to speak to and for this academic body, it has seemed that some considerations on academic ideals might serve to awaken thought and to arouse zeal appropriate to the occasion. In the abstract, however, this would appear to be a delicate and a difficult subject; delicate because of diversity of sentiment, and difficult because of diversity of judgment, amongst those best qualified to speak, as to what academic ideals are, or as to what they should be. Hence it may seem fitting at the outset to suggest application to the views here set forth of the Socratic caution that they can hardly be exactly as represented, if not the more sweeping caution of Marcus Aurelius—'Remember that all is opinion.' But the delicacy and the difficulty of the subject

* Address read on the occasion of the opening exercises of Columbia University, September 28, 1904.

are probably more apparent than real to us; for this is a university assembly, and it is one of the highest functions of a university to examine the various aspects of debatable questions without suppression of candor and without loss of humor.

The typical American university of our time is a complex organization which has grown up rapidly from the typical American college of a half century ago. It has its undergraduate, its professional and its post-graduate schools, as we see them in Columbia University to-day. It has a heterogeneous aggregate of students animated by a great variety of aims and purposes. Its curricula embrace courses of study and research quite unknown to the educated public of thirty or forty years ago; and its degrees recognize professions quite unheard of before the middle of the nineteenth century. Moreover, the modern American university has broken to a large extent with custom and tradition. It is an institution characterized by intellectual agitation, by adjustment and readjustment, by construction and reconstruction, the end of which is not yet in sight. This complex organization is the resultant of the more or less conflicting educational activities of our times. It is a resultant due in part to world-wide influences; it expresses a generalized academic ideal.

Whatever may be our inherited prejudices or our calmer judgments, the attainment of this ideal must be regarded as a remarkable achievement. Here, for example, in this institution, we find all kinds of subjects of study, from the most ancient to the most modern, from the most practical to the most theoretical, from the most empirical to the most scientific, from the most materialistic to the most spiritualistic, all on a plane of intellectual equality and all equally available to those fitted to pursue them. Little surprise is manifested at the close juxtaposition of a professor of metallurgy and a professor of metaphysics, and it has actually been demonstrated that professors of poetry and professors of physics can dwell in peaceful activity under the same roof. Here too the ten or a dozen faculties and the various student bodies mingle and intermingle in a spirit of cooperation and mutual regard almost unknown outside, and hitherto little known within, the academic world.

The mere atmosphere, then, of a modern university must energize and elevate all those who come within its influence. But the domain of this atmosphere is not bounded by academic walls. It is not a limited medium within, but is actually a part of, the unlimited medium of the intellectual world; for the modern university has broken also with custom and tradition in allying itself closely with the external world of thought. Through interaction of the intramural and the extramural spheres of thought the instructor and the student are kept face to face with the vantage ground of contemporary life, whence they may look forward as well as backward.

The modern university is an institution of learning in the full sense of the word; an institution wherein instructors teach students, and wherein, reciprocally, to a very important degree, students teach instructors; for that instructor is fossilized who does not learn more per year from his students, if they are worthy of the name, than they learn from him. Together they work diligently not only to become acquainted with the known, but still more diligently to penetrate the secrets of the unknown. Among them there is a sentiment that condemns alike the instructor who would impart knowledge by the method of the rotary calabash, and the student who, with saturnine stolidity, would absorb only the information poured into his ears. Dwelling thus at a university, not apart from, but actually in, the world of

contemporary thought, students may best fit themselves for the world of contemporary life; and while they may justly esteem it a great privilege to graduate from an historic college, or from a professional school of international reputation, they should esteem it a far higher privilege to graduate from a great university.

It should be observed also that the resultant ideal which has been attained in our best universities is not fixed but progressive, not inflexible but subject to improvement. It is a development whose sources are seen in the earliest civilizations, whose growth was dimly perceived during the middle ages, and whose conscious appreciation is a realization of the century just past. The method which characterizes this development is the method of science. It dates essentially from the epoch of Galileo and Huygens. It rose to a maximum of brilliancy in its interpretation of material phenomena during the epoch of Newton and Leibnitz, and during the epoch of Laplace and Lavoisier; and it has recently illuminated a new domain through the labors of Darwin and Spencer. Galileo, Newton and Laplace gave us a system of the inorganic world; Darwin and Spencer have given us a system which includes the organic world as well.

The method of science has permeated all regions of thought and animated all of the commercial, industrial, political, social and religious activities of men. Whether we welcome it, deplore it, or indifferently acquiesce in it, the fact seems undeniable that the method of science and the doctrine of evolution are the most effective sources of the intellectual enterprise of our day. Through anthropology this method and this doctrine have given a transcendent interest to the study of man; for they show that man may not only investigate the rest of the universe, but that he may, by the same means, investigate himself. Consciously or unconsciously, the terminology, the figures of speech and the modes of thought of science are being applied to all subjects and objects of human concern. They have penetrated the depths and the darkness even of the polite literature of our times.

But while the ideal thus outlined appears to be the effective, or working, ideal at which we have arrived, it goes without saying that it is not the only ideal entertained by those whose opinions on academic questions are worthy of regard. On the contrary, many eminent minds deplore present tendencies and write and speak regretfully of the vanishing ideals of the past. Grave publicists, accomplished men of letters and subtle philosophers see little but danger in the educational readjustments of recent times. They deplore especially the decline in popularity of those ancient studies long called the humanities and the contemporary rise and increasing recognition of the newer studies. Culture, they seem to claim, comes inevitably through the pursuit of the former, never through pursuit of the latter. They go so far in some cases as to decide at what point the study of a subject ceases to be liberal and begins to be illiberal, or professional. Give a student by the ancient formula, their facile editors say, that modicum of learning which would otherwise be dangerous, stamp him with the degree of A.B., and he becomes an aristocrat. They take a gloomy view of the restless present and they are little hopeful of the future; for they hint darkly of 'the bankruptcy of science' and of disasters impending if we do not return to ancient ideals.

Argument concerning these matters is fruitless. Logic avails as little in an educational campaign as political economy avails in a presidential campaign. Appeal must be had to our sense of humor and to the arbitrament of time. It may be observed, however, that these apostles of

doubt and prophets of evil are slowly disappearing. They are more numerous outside than inside academic walls, they are less strenuous in large than in small colleges, and they are no longer dominant in the best universities. From a philosophic point of view they illustrate the action of a most interesting and usually beneficial sociological principle. When consciously applied this principle may be called the law of rational conservatism. When unconsciously applied it may be called, in analogy with a great physical principle, the law of conservation of ignorance. It is so much more important for society to protect itself against the follies of the unwise than it is to profit by the improvements of the wise, that progress comes, generally, only painfully slowly. May we not entertain the hypothesis that the contemporary opponents of educational reforms have been animated towards them rather unconsciously than consciously? Having drunk deeply at certain fountains of learning, they appear to be sure that there are no others. They seem to have been, and to be, always receding. For more than a thousand years, in fact, the gaze of most scholars has been fixed so steadfastly on the glories of the past that it has been possible to advance only by marching backwards.

Through the unconscious action of the law of the conservation of ignorance we are always in danger of disproportionate estimates of educational values and of erroneous judgments in the larger affairs of life. We involuntarily revert to precedent, commending what is old, condemning what is new. Thus, to give a concrete illustration, fear and panic would be visible in our faces if we did not understand the mythical significance of the names Phobos and Deimos lately applied to the moons of the planet Mars; but very few of us would betray the slightest mental disquietude at our profound lack of knowledge of the properties of the atmosphere which is the medium of communication between you and me in this room. Thus, also, in spite of the obvious aphorism that all men are human, they have been divided into humanists and non-humanists, Matthew Arnold, for example, being one of the former, and the founder of our John Tyndall Fellowship being one of the latter. And stranger still, one might infer from the slowness of legal and constitutional reforms, and from many current arguments opposed thereto, that laws and constitutions are not made by men for men, but that, in some mysterious way, men are merely experimental material for the training of crafty lawyers and sagacious politicians.

But we have broken irrevocably with the past; not in the sense of disregarding the rich heritage of experience from our distinguished predecessors, but in the sense that their customs and traditions no longer dominate us. We have corrected their observations for geocentric parallax; and we must now correct their observations for anthropocentric parallax, just as our successors, if they prove progressive, will surely correct our blunders and avoid our errors. The need of corrections for anthropocentric parallax in educational affairs is now widely recognized. It leads to the investigations of Mosely Commissions, to the conferences of the Association of American Universities, and to the broader conferences of world's fair congresses. It is the chief source of the educational activities of our day. In these activities are to be seen the most hopeful signs of the times; for while agitation does not necessarily mean progress, serene contentment is pretty certain to mean stagnation, if not regress.

And the readjustment now going on in the academic world must continue. It is a part, simply, of the readjustment going on

in the intellectual world at large. We are, so to speak, in a state of unstable equilibrium, wherein mental repose can be purchased only at the price of mental somnolence. Great as have been the enlargement and the appreciation of educational and professional opportunities during the past three or four decades, we may confidently anticipate still wider enlargement and appreciation in the future. New divisions of knowledge may be expected to arise, and old divisions may be expected to undergo marked expansion, redistribution or emendation. The so-called humanities, especially, must be broadened, purified and elevated if possible to the intellectual level of the more highly developed sciences. It is clear, indeed, that in any revision of the humanities some matters may be redistributed, if not discarded, with advantage. The reckless amours and the clandestine peccadilloes of ancient and modern royalty, for example, should be transferred from the historian and the novelist to the anthropologist, the alienist and the pathologist. Such humanities, and many others of like kind, can hardly stand in comparison with the constancy of the stars and the beauties of harmonic analysis.

All these matters of controversy, however, belong rather to the lower than to the higher life of a university. How a student acquires elementary training is an academic question in the narrower sense of the word. The world cares little for educational ways and means unless they can commend themselves by results. Attainments must be tested by achievements and proficiency must be proved by progress. To rise to this standard of excellence is the ideal of the higher life of a university. It is only by the pursuit of, and in the realization of, this ideal, that instructors and students may keep pace with and contribute adequately to the advancement of modern knowledge. Those who would separate theory from practice, those who would draw lines of invidious distinction between pure and applied science, along with those who would mistake a part of archeology for the whole of education, are all alike inimical to the trend of current progress.

It is the highest function of a university to cherish this ideal and to promote especially the arduous labors essential to fruitful original research. Those who can add somewhat to the sum and substance of permanent knowledge by the establishment of a physical, a social, an esthetic or an ethical principle, are the greatest benefactors of our race. Of the many who feel drawn to this high calling, however, few are destined for fame. Only those who prefer the turmoil of conflicting thoughts to the tranquility of inherited opinions, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, may hope to attain renown. But, as those serve also who stand resolutely and toil patiently at their allotted tasks, so is there room in the grand aggregate of human achievement for the humblest as well as for the noblest of investigators.

The ideals, then, of a modern university, like the ideals of the intellectual world at large, contemplate achievement and progress in all grades of work from the lowest to the highest. They demand endless patience and unflagging industry from all who seek to rise above the dead level of mediocrity. The opportunities now afforded for the pursuit of, for the acquirement of, and for the advancement of, learning are greater than ever before. We are the heirs of the ages. But along with an increasing heritage there come increasing duties and increasing responsibilities. It rests with us to show that we are worthy of this heritage and able to meet these duties and responsibilities. This is the line of endeavor we resume to-day, and the spirit

of the hour bids us look forward with cheerful optimism.

R. S. Woodward.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.
THE PRESENT STATE OF GEODESY.*

The problems of geodesy, like those of most sciences, enter upon new phases with the accumulation of facts bearing upon them. The problem of determining the amount of the earth's compression was added to that of determining the size of the supposed sphere as soon as Newton had demonstrated its oblateness. The controversy to which Newton's theory gave rise was settled by the famous geodetic operations of the eighteenth century which furnished the cardinal facts in regard to the earth's figure and size.

What may be regarded as the slow progress of a more precise knowledge of the earth's dimensions since that time must be attributed to the difficulties inherent in the problem.

In the first place the dimensional measurements must necessarily be confined to the continental areas which occupy but three elevenths of the earth's surface. The configuration and relationship of these areas make it impossible to girdle any section of the earth by direct measurement.

Secondly, the admeasurement of these areas is far beyond the reach of individual enterprise and can only take place when the practical needs of governments suggest the utility of great mensurational surveys which at the same time and without great additional expense will furnish the data required for a more perfect knowledge of the spheroid. In making this statement it is not forgotten that individuals and governments did undertake in all ages measurements for the purely scientific purpose of determining the size of the earth, for the desire for knowledge on this subject may be reckoned coeval with intellectual development of man.

Happily it may be said also that by their collective action the governments of the world have shown in recent times that it is considered a governmental function to support and promote researches in this branch of science. I allude, of course, to the existence of the International Geodetic Association. It will not be out of place to say in this connection that the association exists by virtue of a formal convention between the participating governments, which are, at the present time, the United States, Japan and Mexico and all the European nations save Portugal, Roumania and the group south of the Danube. No account of geodesy would be complete that failed to consider the aims and labors of this association. Its history is part of the history of geodesy since 1861. At that time it began its career as the Mittel Europäische Gradmesung. In a few years it expanded into an European association and in 1886 it became international.

It is not generally known that it was this association which instigated the French government to invite the world to establish an international bureau of weights and measures at Paris. Without detracting in any way from the labors of Bessel, Clarke and others in intercomparing geodetic standards, the successful labors of the bureau which in consequence was established in Paris removed at least some of the difficulties that were encountered by the investigators in this branch of science, and by those engaged in the practical work of the measurement of the earth.

The history of geodesy is full of instances of confusion and wasted energy due to the lack of a common standard, and the results of many are measures which would

* Address of the vice-president and chairman of Section A—Mathematics and Astronomy, American Association, Philadelphia, December, 1904.

at least have great historic interest are utterly lost to us, because we can not make even a respectable guess at the units used. The adoption of an international unit of length and its necessary auxiliary, a common thermometric scale, and the provision which the various governments made for the reference of their measuring apparatus to a common unit was a step of fundamental importance.

The association as such has no control over the geodetic operations conducted by the different governments. Its function is to be the intermediary where cooperative action is needed, and to discover and point out along what lines the greatest need for information exists.

In pursuance of these duties it has helped to perfect the European systems of triangulation by showing where missing links should be supplied, not only by measurement of angles and bases but also by additional astronomical observations. It has made absolute gravity determinations with all the accuracy demanded by modern science and has caused suitable connection to be made by relative measures between widely scattered pendulum base stations, and it has instituted unique relative gravity measures, to which further reference will be made. It organized and maintains the stations for observing the variation of latitude in regard to which it should be remarked that it is the desire of the association to continue the observations beyond the year 1906, which marks the end of the ten-year period for which that service was tentatively organized. The association strongly desires not only to continue but to extend the service to the southern hemisphere and other latitudes than those now occupied by the permanent stations, and to obtain the cooperation of suitably situated observatories in their endeavor to discover the cause of the phenomena.

That the problem of determining the earth's dimensions could not be solved by simply measuring two arcs in suitable localities was brought home to geometers by the anomalous results obtained in the eighteenth century. For instance, according to Lindenau the combination of the two American arcs, Mason and Dixon's, measured in 1764, and that of Peru, measured a quarter of a century earlier, gave a value of one five-hundredths for the earth's compression. The value derived from those measured in Great Britain alone was about nine times as great, or one fifty-fifth, while those made in France, considered by themselves, gave one one-hundred-and-fiftieth. It is not now important to inquire whether these differences are not in part due to the crudities of the methods of measurement employed. They were sufficiently real to throw doubt on the belief that the earth could be represented by a regular mathematical figure. Finally, the existence of local deflections of the vertical as affecting the amplitude of arcs was recognized, but not taken into account save, perhaps, by arbitrary exclusion of stations showing exceptionally large deflections.

The method of finding an osculating spheroid from arc measures remained in its essence that of taking averages of measurements reduced to the geoidal surface. The differences between the observed directions of the vertical and those computed on an assumed spheroid of reference were treated as if they were accidental errors of observation. At the present time it is the aim of geodesists to assign to the deflections their proper place in the computations and to interpret them by discovering through them and through gravity measurements the manner of the distribution of masses in the interior of the earth. Thus geodesy is trenching on the domain of geophysics and geology.

In India, in Europe and in the United States the study of these deflections is re-

ceiving special attention. In the last-named countries the junction and correlation of the triangulation, which was formerly disjointed, makes it possible to take up the study. Similarly in this country the completion of the transcontinental arc and its connection with the lake survey triangulation furnished the opportunity and occasion for adopting a standard datum of geographical coordinates for the whole country. This in turn furnished the deflections of the vertical referred to a common origin of coordinates on the same spheroid and made it possible to begin the study of the form of the geoid in this country over a very extended area.

The investigation has so far been extended over the eastern part of the United States. Here as elsewhere it was found that the curves of elevation of the geoid above the spheroid reflect perceptibly the visible topographic features.

A preliminary statement of the scope of these investigations was recently given before the International Geographic Congress by Mr. Hayford, the chief of the computing division of the Coast and Geodetic Survey. From it I quote as follows:

> The conclusion that for the eastern half of the United States and the adjacent portion of the Atlantic the theory of isostasy is true to a considerable extent is reasonably safe. The conclusion that the depth within which the isostatic compensation takes place is 205 miles is one which may be modified considerably as the investigation proceeds.
>
> The investigation thus far leaves the signs of the corrections to the constants of the Clarke spheroid of 1866 uncertain.

Mr. Hayford will give before this meeting an account of the method devised by him of computing the topographic correction. The task of computing this correction to a distance of 4,000 kilometers for each of say 500 stations has been rendered possible by this method, which is, therefore, referred to by me as a distinct advance in geodesy.

It is hoped that the completion of the study of the data now available in regard to the deflections will serve as a guide to the most effective use in the future of the pendulum, and it is on this account largely that pendulum observations have been for the present deferred by the coast survey. They are, however, being actively made by other nations.

A new impetus was given to relative gravity observations by the adoption of short and light pendulums in place of the heavy seconds' pendulum. Aside from their portability, their lightness insures greater invariability for the knife edges, simplifies the task of securing uniformity of temperature and pressure in the metal cases in which they are swung, and the ease with which a low and constant pressure can be maintained in the case insures the continuance of the swing through so long a period that the errors of the chronometer or other timepiece are eliminated. Thanks to the efforts of the International Geodetic Association, the widely scattered base stations have been connected with the central station of the association at Potsdam, where a long series of absolute gravity determinations were brought to a successful conclusion two years ago. The association has available now the data from nearly 1,800 stations scattered over various parts of the globe. A most interesting and valuable extension of relative gravity measures to the surface of the ocean was made two years ago. The principle upon which the new method depends is that if the atmospheric pressure is determined at the same time and place by means of a mercurial barometer on the one hand and by the temperature of the boiling point of water on the other, the observed height of the barometer will be affected by gravity at the place, while the result by the hypsometer will be independent of it. According to Dr. Hecker, who carried out the laboratory

experiments as well as the actual test at sea, the suggestion that the two instruments might be used for the determination of differences of gravity was first published in 1894 by Dr. Guillaume, of the International Bureau of Weights and Measures at Paris. Dr. Mohn, of Christiania, successfully applied the method by actual tests in various places in Norway for the purpose of determining the gravity reduction of the barometer for meteorological purposes. Doctor Hecker installed his apparatus on a steamship and sailed from Hamburg to Rio Janeiro via Lisbon, Portugal, and Bahia, Brazil, and returned on another steamer to Lisbon, making observations both ways. The results of his observations have been published and show:

1. That the intensity of gravity on the Atlantic Ocean between Lisbon and Bahia is nearly normal, and agrees with the theoretical values computed by means of the general formula published by Helmert in 1901.

2. That the difference of gravity at sea in shallow water and in deep water corresponds approximately to the difference of gravity between coast stations and inland stations.

These results were submitted to the Geodetic Association at the last meeting. Means were provided for another expedition and last March Dr. Hecker began his journey, crossing the Indian Ocean and the Pacific by way of Melbourne and Sydney to San Francisco. Thence he recrossed to Japan and China, and we may look forward to an early statement of the results, which are being awaited with deep interest.

As in the case of the pendulum already referred to, there has been in the last decade a decided improvement and simplification in instrumental means and methods of work. It is only necessary to cite the introduction of tapes and wires for primary base measurement, the introduction of the transit micrometer for the elimination of personal equation in time determinations, and of the leveling instrument, devised in the coast survey, which is making its way into more general use. With the use of the latter there has just been satisfactorily completed the first precise line connecting the Atlantic, Gulf and Pacific mean sea levels in the coasts of the United States.

In all countries the determination of the mean sea level and the establishment of so-called bench marks in the interior are being actively prosecuted as they furnish part of the required geodetic data.

In beginning I referred to the measurement of continental areas. Let us see what has been accomplished as to the extent of areal measurement since Snellius introduced triangulation into geodesy 289 years ago. In our own hemisphere, so far as I am able to learn, about the three-hundredth part of one per cent. of the area of South America has been covered; of Mexico about one per cent.; of the United States about five per cent. Geodetically the British possessions in the western hemisphere are barren. We may say that less than three per cent. of the western hemisphere has been triangulated.

In the eastern hemisphere we find that about forty per cent. of Europe has been covered, but if we leave out Russia the percentage rises to eighty per cent. for the rest of Europe.

The triangulation of Asia is furnished by India and Japan, Java and Sumatra and amounts to about four per cent.

Australia shows about two per cent., Africa about two and six tenths per cent., making a total for the eastern hemisphere of about seven per cent.

If we exclude the north and south polar regions a little over six per cent. of the available land area has been triangulated, or about one and one half per cent. of the total surface of the globe. These figures

are accurate enough for the purpose for which they were compiled, that is, to show the relatively small area covered. There is, however, another side to the picture, the hopeful one. In South America the arc of Peru is being remeasured and extended by the French government. As the work is being carried out with the advice of the most distinguished mathematicians of France, the results will be, in their importance, out of all proportion to the extent and area involved.

Mexico has made a brave beginning and is working towards a connection with an extension of the ninety-eighth meridian measurement, of which the United States has completed about three quarters of the amplitude lying in her own domains. Work on the Pacific coast arc has been resumed and it has nearly been completed from San Diego to the Columbia River.

Two years ago the Russians and Swedes jointly completed an arc in Spitzbergen between latitudes 76° and 81°. The European arcs are being extended eastward by Russia, and one must look forward to the ultimate connection between the Russian triangulation at Astrakhan or Orsk and the Indian triangulation, however improbable it may seem if looked at from a political view point.

In Africa the work of extending the South African arcs northward from the Cape towards Alexandria is well under way, and no doubt need be entertained that the British and Germans will carry it through.

A general review of this part of the field of geodesy shows that while some great geodetic measurements have been completed or are approaching completion, new ones are being undertaken under the fostering care of different governments.

Reasoning from the experience of the past, we may conclude that the solution of one problem in geodesy will disclose the existence of another, and from the trend of the investigations of the present that other than purely mathematical and astronomical sciences will be advanced by the search for their solution.

That the progress of the branches of science to which this section of our association devotes itself was greatly affected by the problems of geodesy was pointed out by Humboldt in language which may fittingly conclude these remarks:

> Except the investigations concerning the parallax of the fixed stars, which led to the discovery of aberration and nutation, the history of science presents no problem in which the object obtained—the knowledge of the mean compression of the earth and the certainty that its figure is not a regular one—is so far surpassed in importance by the incidental gain which, in the course of long and arduous pursuit, has accrued in the general cultivation and advancement of mathematical and astronomical knowledge.

O. H. Tittmann.
U. S. Coast and Geodetic Survey.

---

*FUTURE DEVELOPMENTS IN PHYSICAL CHEMISTRY.**

It has been the custom of the retiring officers to discuss the development of some portion of that field of chemistry in which they were most interested. Since the president of the American Chemical Society will speak on physical chemistry tomorrow night, it has seemed to me that I might break with tradition and discuss the future of physical chemistry rather than its present or its past.

We have reached a critical stage in the development of the electrolytic dissociation theory. The work of Kahlenberg has shown that there are a number of facts which we did not anticipate and which we can not explain satisfactorily at the present time. The recent experiments of Noyes show that the dilution law does not hold for any strong electrolyte and that the

* Address of the vice-president and chairman of Section C—Chemistry, Philadelphia, 1904.

same empirical equation describes the behavior of binary and of ternary electrolytes. This last fact appears to be fatal to all explanations based on the assumption that electrostatic effects are the disturbing factors. While the mutual attraction or repulsion of two ions or of three ions may easily change the dissociation formula for a binary or a ternary electrolyte, it is very improbable that the changes will be such as to make two radically different formulas identical. Of course, the hypothesis of hydrated ions gives us some leeway but the outlook is not what it was five years ago. It is too soon yet to say whether we are merely to remodel the electrolytic dissociation theory or whether we are to replace it by something else. My own opinion is that reform is what is needed and not revolution. It is evident, however, that we have gone ahead too fast and that we must test more thoroughly the premises on which our conclusions are based. We know of one error. The proportionality between molecular weight and osmotic pressure holds only for the cases in which the heat of dilution is zero. This is stated clearly in van't Hoff's original deduction of the van't Hoff-Raoult formula $n/N = \log p/p_1$, but has been pretty generally overlooked. Since the heat of dilution is rarely zero in any actual case, our deductions as to the molecular weights of solutes are always somewhat in error. In the case of the metals of the alkalies and the alkaline earths dissolved in mercury, the lowering of the vapor-pressure due to the heat of dilution is practically equal to that due to the molecular weight, and we therefore have the surprising result that the apparent molecular weight is only about one half the atomic weight. One of the first things we have to do is to eliminate this source of error in all cases.

Another distressing feature in the quantitative physical chemistry of to-day is that the field which it covers is daily growing less. A tenth-normal solution is now considered a concentrated one, and some people are so extreme as to maintain that we can not expect agreement between theory and experiment for anything except infinitely dilute solutions. To my mind a theory which holds only for infinite dilution is necessarily wrong. Here again one probable source of error is easy to find. The van't Hoff-Raoult formula is deduced on the explicit assumption that there is no specific attraction between solvent and solute. If this assumption is wrong, it is reasonable to suppose that the error thus introduced would become less as the concentration approaches zero. Under these circumstances the van't Hoff-Raoult formula might represent the facts at infinite dilution without being a true formulation. This is the case with another well-known and important formula. The Helmholtz and the Nernst equations for the electromotive force of concentration cells are identical for infinitely dilute solutions and for these only. The Nernst equation ignores the concentration of the undissociated salt, while the Helmholtz formulation does not. The two equations become identical at the moment when the concentration of the undissociated salt and the disturbing factor due to it become zero, that is, at infinite dilution. Since the Helmholtz formula applies to all concentrations, the Nernst formula is necessarily only approximately accurate. This has been recognized explicitly by Planck, though the point is often overlooked. It is quite conceivable that the shortcomings of the van't Hoff-Raoult formula may be due in part to theoretical inaccuracies and that we have laid too much stress on 'variations from the gas laws.'

If we introduce the conception of a specific affinity between solvent and solute in certain cases, notably those in which the

heat of dilution is marked, we combine all of what has stood the test with what is good in Kahlenberg's conceptions and I believe that we are nearly ready to take a long step forward. One point must be kept in mind, however. Raoult's experiments preceded his formula. Before we can hope to work out a satisfactory theory of concentrated solutions, we must have accurate measurements on concentrated solutions and at present we have practically none. We need experiments at constant temperature on the compositions of coexistent liquid and vapor phases for binary systems with one volatile component and with two volatile components. These measurements are not easy to make and that is one reason why they have not been made. We have measured boiling-points and freezing-points because they are easy to measure; but for a theory of concentrated solutions the value of such measurements is very small. This is because we are then measuring the combined effect of the change of the pressure with concentration and with temperature, whereas we ought to study the two separately. Further, if we are to express our results in volume concentrations we must give the volume concentrations of both components. It would be absurd to pass from dilute to syrupy solutions of sugar, for instance, and to treat the concentration of the water as constant. Personally, I believe that the theory of concentrated solutions is relatively simple and that the difficulties have been chiefly of our own making. My own experience with ternary mixtures confirms me in this view. In developing a theory of concentrated solutions we must also keep in mind the actual properties of the components, a thing which we have not done in the past. Thus the dissociation equation for liquid chloral hydrate can not be the same as that for liquid chloral alcoholate because chloral is miscible in all proportions with alcohol and forms two liquid layers with water. This is a perfectly obvious fact, yet no reference to it is to be found in any text-book on physical chemistry.

In the last ten years the work of Roozeboom and others has brought the phase rule to the front as a basis of classification and as an instrument of research. The importance of the phase rule is going to increase very rapidly in the next decade. The study of alloys has really only just begun. Our knowledge of the carbon steels is still very incomplete and unsatisfactory. In fact, we know the constitution only of a very limited number of binary alloys. Nothing systematic is yet known about the chemical properties of alloys or about the conditions for electrolytic precipitation. The variation of the engineering properties, such as tensile strength, torsional resistance, ductility, etc., with varying concentration and varying heat treatment is a subject which can only be worked out satisfactorily with the phase rule as a guide. On the basis of what has been done it appears quite safe to predict that we do not yet know one half the possibilities of our structural metals.

Quite recently the constitution of Portland cement has been established and we owe this result to an application of the phase rule. It will not be long now before we get much clearer ideas on the causes of the strength of cements and of the plasticity of the clays. The time will soon come in our engineering schools when the subject known as 'Materials of Engineering' will have to be taught by the chemist rather than by the engineer.

The applications of the phase rule to petrography will be numerous and will come soon. It is evident that no rational classification of minerals can be possible until the constitution of the minerals has been determined. The situation in regard

to petrography is much the same to-day as it was in regard to alloys a few years ago and we may reasonably expect as satisfactory results from rocks as from metals. More and more people are experimenting with fused salts and the new geophysical laboratory at Washington is planning to study igneous rocks in the same thorough way that van't Hoff studied the Stassfurt deposits. The problem is a difficult one experimentally, but it can and will be solved.

The classification of electrochemistry under the phase rule is a problem of the immediate future. Some work has been done already, but it is confined to the discussion of the electromotive forces of certain reversible cells. What I mean is something vastly wider than this, the application of the phase rule to all electrolytic and electrothermal processes. Since electrochemistry is essentially chemistry, a classification which is of fundamental importance in chemistry must be equally necessary in electrochemistry.

The extension of the phase rule to organic chemistry is an achievement about which we like to dream, but the realization of it seems far off. To treat a large portion of organic chemistry as a system made up of carbon, hydrogen and oxygen will some day be possible; but at present we are balked by so-called 'passive resistances to change.' Theoretically methyl ether, $(CH_3)_2O$, and ethyl alcohol, C_2H_5OH, are two modifications of the substance C_2H_6O and they should be mutually convertible. Practically they are not. Only one of the three dibrombenzenes can theoretically be the stable form. Actually, we can not convert any one of them directly into either of the other two.

In spite of all this there is really quite a mass of material waiting to be worked up. Reversible equilibrium between hydrogen and oxygen can be realized at all temperatures. Reversible equilibrium between carbon, carbon monoxide and carbon dioxide is possible above 200°, while reversible equilibrium between carbon, methane, acetylene, ethane and hydrogen can be observed above 1200° without catalytic agents. Carbon monoxide and water react at 430° in presence of copper. Methane can be made from carbon monoxide and hydrogen at 250° in presence of nickel, while methyl alcohol can be changed to carbon monoxide and hydrogen by zinc dust. The decomposition of alcohols into aldehydes, or ketones, and hydrogen is reversible. Aldehydes can be changed into carbon monoxide and paraffines, though the reverse reaction has not been accomplished satisfactorily. Methylal and acetal are formed by a reversible reaction, while the ester formation has been studied for years. Formic acid decomposes into carbon monoxide and water when heated by itself, and into carbon dioxide and hydrogen when heated in presence of rhodium. Starting from carbon monoxide and caustic soda we can make sodium formate, carbonate and oxalate.

As yet only a few of these reactions have been studied with care and we do not know how many of them are reversible or what are the temperature limits. We do not even know whether colloidal metals act more effectively than the pulverulent metals, although it is very probable that they do. While we can not yet tell how far we may be able to go, it is clear that the attempt to apply the phase rule to organic chemistry opens up a most interesting field of research both as regards organic chemistry and as regards the theory of catalytic agents.

The usefulness of the phase rule in studying basic and double salts is being realized more and more by our friends the inorganic chemists. The recent work on the changing solubility of the hydroxides of

many of the metals calls attention to a possibility of error which must not be overlooked. In all cases of hydrolysis there is always a possibility that equilibrium may not be reached in weeks or months. The only safe way is to reach the equilibrium from both sides. In this way and only in this way do we get any clue to the magnitude of the error involved and it is only after we have done this that we are justified in assuming that a reaction is irreversible.

The application of the phase rule to the fractional crystallization of rare earths would certainly lead to marked improvements. There are few people who could separate potassium and sodium chlorides by fractional crystallization, getting out all of each salt entirely pure. Even fewer would be able to separate potassium sulphate and copper sulphate. In spite of this we start in cheerfully on the fractional crystallization of an unknown number of elements having unknown properties. The result of all this is that we reach a point where further separation is impossible and yet we do not know why. This state of things is really the fault of the physical chemist and not of the inorganic chemist. It is not to be expected that the inorganic chemist can start in off-hand and apply the phase rule to the study of basic and double salts or of rare earths. Before this can be done the physical chemist must work out the methods and must be prepared to give explicit working directions, possibly in the form of recipes.

It must also be clear to you that a study of the conditions of existence of compounds, atomic and molecular, is a prerequisite to any theory of valency,

In the past, reactions in organic chemistry have been studied by physical chemists chiefly as examples of reaction velocity. There are two other fields which will receive more attention in the near future, namely, yields and irreversible reactions. The question of yields is in a very bad way. In Lassar-Cohn's admirable book on laboratory methods in organic chemistry there is an enormous amount of valuable material; but there is really very little in the way of theory. Although we know that a reversible reaction will run to an end if the concentration of one of the reacting substances be kept practically zero, surprisingly little use has been made of this principle. We know that certain reactions take place better in dilute solutions or at low temperatures or in certain solvents, but in most cases we can not tell why. In the pyridine method for introducing acetyl or benzoyl groups the pyridine is said to be effective because it is a weak base; but it is much more probable that it acts as a catalytic agent. We do not know how far the dehydrating action of certain reagents is simply a question of vapor-pressure or how far there is a specific effect due to the particular reagent. The action of sulphuric acid in the formation of ether is something more than a dehydrating effect, and the same is true of the effect of zinc chloride in the synthesis of ethyl chloride.

A single instance will be sufficient to show the state of confusion that exists. Anschütz's method of preparing certain esters was to saturate the solution with hydrochloric acid gas and to allow the solution to stand overnight. Fischer improved on this by adding less acid and by raising the temperature. He boiled for two hours and found that the hydrochloric acid concentration could be reduced to three per cent. without affecting the yield. There the matter is left and we are led to look upon a three per cent. concentration as having special merits, whereas this is undoubtedly merely a result of boiling for the arbitrary period of two hours. If Fischer had boiled for one hour only he would have had to use a stronger acid to

have reached equilibrium in the allotted time. If he had boiled three hours, the lowest permissible concentration of hydrochloric acid would undoubtedly have been less than three per cent. Anschütz, on the other hand, worked at ordinary temperature and his solutions consequently needed more acid and more time to approximate to equilibrium. All of this is really first principles and it is only one case out of many. If any one will try to classify and explain the results given in Lassar-Cohn's book, he will find himself provided with enough interesting research to last him the rest of his natural life.

The second field for research to which I have alluded is that of irreversible reactions. In inorganic chemistry there are as yet no well-authenticated cases where a reaction starts and then stops short of equilibrium. The results of Pélabon on hydrogen and selenium and of Hélier on hydrogen and oxygen have been disputed by Bodenstein and must for the present be considered as wrong. In organic chemistry we appear to have many such reactions, typical instances being the formation of nitro-benzene and the decomposition of aldehyde into methane and carbon monoxide. While it is possible that these and other reactions run to an end in infinite time, we have not infinite time at our disposal, and it may, therefore, prove profitable to find out whether and how the apparent end-point varies with varying initial conditions. This work is desirable now and will become necessary if we should ever revise our opinions as to the theoretical possibility of an irreversible equilibrium. By definition we can not determine the existence of an irreversible equilibrium by approaching the end-point from the two sides. It seems to me probable, however, that we can draw conclusions from the reaction velocity. If we are dealing with a case of a theoretically reversible reaction running practically to an end, I can see no reason why the concentration of the decomposition products should have any effect on the reaction velocity, so long as we confine ourselves to gaseous systems. If, however, we are dealing with a theoretically irreversible reaction which does not run to an end, the reaction velocity would vary with the concentration of the decomposition products.

It should be noticed that it will not do to reason from the behavior of a system in presence of a catalytic agent to that of a system without a catalytic agent, since the catalytic agent may displace the equilibrium. Thus ethyl alcohol is decomposed by heated copper into aldehyde and hydrogen, while heated alumina changes it chiefly into ethylene and water. It was the study of organic solutes in organic solvents which led Raoult to the formulation of his law. It seems probable that a study of organic reactions may lead to an entirely new class of equilibria. If this happens it will throw much light on the preceding problem because it is very difficult to explain some of the peculiarities in regard to yields in organic chemistry so long as we are obliged to postulate reversible reactions only.

The theorem of Le Chatelier has been applied chiefly to heat and work effects, but this is by no means the extent of its usefulness. Wherever we get a reversible displacement of equilibrium by light, it must be possible to make use of this theorem. The change of color of the silver photochlorides is in accordance with the theorem; but there seems to be no reverse change in the dark. The simplest case with which to begin would appear to be the formation of ozone. There seems to be a contradiction here. Ozone is known to absorb ultra-violet light and yet it is believed to be formed by the action of ultra-violet light. Whether we are dealing with the same sets of rays in the two cases is a point that has not been settled. In fact, we do

not know definitely whether ozone is formed by the action of ultra-violet light in the absence of electrical waves, though this is a matter easily settled by experiment. We know that ozone gives out light on decomposing, but we do not know anything about the spectrum of this light. It is quite probable also that we must formulate the theorem of Le Chatelier more exactly than we have hitherto done before we can apply it successfully to the phenomena of light. An instance based on electrical phenomena will show what I mean. If a voltaic cell be short-circuited the chemical change will be such as to decrease the electromotive force of the cell. If we do not keep the cell at constant temperature the Joule heat will cause the temperature to rise and this may either raise or lower the electromotive force of the cell. We are then really considering two phenomena, the electrical and the heat effects. One may mask the other completely.

In one case, at any rate, we know that we can apply the theorem of Le Chatelier to light phenomena. Suppose we have a gas enclosed in a transparent adiabatic vessel and concentrate upon it light of a wave-length that is absorbed by the gas. The temperature of the gas will rise and equilibrium will be reached when the gas has changed so that it no longer absorbs light of that particular wave-length or when the gas emits light of the same wave-length and intensity as that which is acting upon it. This emission by a gas at some temperature of the light which it absorbs at the same temperature is Kirchhoff's law, which thus appears as a special case of what the chemists call the theorem of Le Chatelier. To be frank, I do not now see how we are to apply this theorem to the phenomenon of phosphorescence, and yet we are dealing with an absorption and an emission of light. I venture to suggest that it is to the application of the theorem of Le Chatelier that we must look for a rational treatment of phosphorescence, fluorescence, chemiluminescence, etc., rather than to a theory of vibrating molecules. It will be time enough to discuss the application to radiations when we have solved the simpler problem of the theory of cold light.

A discussion of equilibrium relations would not be complete without some reference to the future of thermodynamics in chemistry. There are two radically distinct ways of considering the relation of thermodynamics to chemistry. One is to look upon thermodynamics as a mathematical shorthand. The aim of thermodynamics is then to present a consistent and formal treatment of the known energy relations. In this case thermodynamics deals with the past and not with the future; with the classification of knowledge and not with the discovery of new laws. This is the point of view of most mathematical chemists and it is because of this that we do not turn to the mathematical chemist for new ideas. There is another way of considering thermodynamics, namely, as an instrument of research. It is not too much to say that the mathematical chemist can work out in a few hours or days results which would take his less fortunate colleague months or even years to obtain. At present the race is to the tortoise and not to the hare; but I can not believe that this will always be so. Other things being equal, the man who can handle his thermodynamics will beat the man who can not; but in order to have that take place thermodynamics must be considered as an instrument of research and not as a branch of metaphysics. We must confess that the mathematical chemistry of the past decade has not done what it should have done and that there is no immediate prospect of any improvement. In the meantime we do not despair. There are great possibilities in

the application of mathematics to chemistry and some day they will be developed.

So far we have considered problems involving equilibrium only. When we begin to study the conditions which make a reaction possible and which govern its rate, we are brought face to face with our need for a satisfactory theory of catalytic agents. We know experimentally the catalytic action of many substances on many reactions, but we have not even the first suggestion of an adequate theory. This is a subject of more vital importance than may appear at first sight. I wish to call your attention to two very important matters which depend directly upon catalytic agents. The first is the chemistry of plants. We can make in the laboratory many of the substances which the plant makes. Some of them, such as alizarine and indigo, we can make more cheaply than the plant can, and of a higher degree of purity. As yet we can not make any of them in the way the plant does, and this gap in our knowledge will have to be filled by the physical chemist, as the problem apparently does not appeal to the organic chemist. The plant does not use reverse coolers or sealed tubes; it does not boil with sulphuric acid or fuse with caustic potash; it has not metallic sodium and chlorine gas as reagents. The reagents on which the plant can draw are air, water and a few mineral salts. As catalytic agents it has heat, light, difference of electrical potential, enzymes—and itself, namely, living protoplasm. From the work of Bredig and others we know that colloidal metals, the so-called inorganic ferments, can be substituted for enzymes in some cases. As we do not yet know our limitations, it is quite possible that we can substitute inorganic catalytic agents for the enzymes in all cases. If that proves to be true we can then duplicate everything except the plant itself, and we shall be ready to determine how closely we can duplicate the reactions of the plant. The experiments of Sabatier and Senderens in France are distinctly encouraging, even though they do not carry us very far. By means of nickel powder it is possible to reduce acetaldehyde to alcohol with hydrogen at 30°. This is the best result that has been obtained and it indicates the possibilities. When we get a satisfactory theory of catalytic agents we shall undoubtedly be able to duplicate many of the plant syntheses and our failures will be interesting as bringing us nearer to the most difficult problem of all—that of life. Pending the development of a satisfactory theory of catalytic agents, there is much to be done in the way of experimenting. In view of the fact that mixtures of two catalytic agents often act more intensely than would be expected from the behavior of each taken singly, it would appear advisable to determine the combined effects of inorganic ferments and ultra-violet light.

The second problem, which would be easier of attack if we had a satisfactory theory of catalytic agents, is that of the transmutation of the elements. This is now admitted to be distinctly a scientific problem, though not one in which we have made much progress. It is usually assumed that it is a very difficult problem. While this may be true, we have not yet reached the point where we are justified in being certain of it. No one has ever attacked the problem systematically and all we can say is that the rate of change has been small under any conditions that we have yet realized. That is not surprising. We should naturally expect a low reaction velocity. The rate of change of radium is so slight that it could not be detected by any ordinary methods. The fact that we have never observed any transmutation of the elements does not prove that none has taken place. We had been making diamonds artificially for years, even for cen-

turies, but nobody thought of looking for them in cast iron until after Moissan made his experiments a few years ago.

If we accept Lockyer's conclusions as to the state of things in the sun, we could undoubtedly break up many of the elements if we could hold them long enough at 6000° C. One difficulty is to get the temperature, and of course we must be cautious about conclusions based on simplified spectra. Many people have thought that radium was to be the catalytic agent which was to change all the elements; but the recent work of Rutherford seems to put an end to this idea. If radio-active lead, tellurium and bismuth are merely these elements plus the radium emanation or one of its decomposition products, there is very little evidence to show that any of our well-established elements are undergoing any change from contact with radio-active substances.

Another possibility which has been suggested is that we could change our elements if we could pump energy into them and change their energy content. This would have to be done electrically if at all. I have been told that Stas was busy during the last years of his life trying to change sodium into something else by an electrical process. The difficulty is to pump energy into the element. Passing a heavy current through a metal produces no effect that we know of other than to raise the temperature. Taking the element in the state of gas enables us to employ a higher potential difference, but here the effectiveness of the method is limited by the appearance of the arc. The first stage in the problem would, therefore, be the attainment of the highest possible potential difference without causing arcing. In view of the remarkable insulating action of gases under high pressure, it seems as though the silent discharge through compressed gases were the thing to try. The difficulties people had in proving the dissociation of water at high temperatures makes us realize the possibility that we might decompose our elements and never know it, owing to the recombination taking place at once. If we are to simplify our elements by pumping energy into them, it appears that we should work with gases under high pressure, with the highest potential difference compatible with the absence of sparking, and with some application of the principle of the hot-cold tube.

While the methods of extremely high temperature and of high electrical stress have much to commend them on paper, they are liable to fail owing to the difficulty of attaining the proper temperature or the proper electrical stress. The ideal method would be to find a catalytic agent which would accelerate the rate of change and which would eliminate what we should then call the instable elements. Since there is no immediate prospect of our being able to predict the suitable catalytic agent and the conditions under which it is to be used, we must ask ourselves what is the scientific method of attacking the problem of the transmutation of the elements.

The answer is a simple one. We must start with the simplest case, study that thoroughly, and work up gradually to the more difficult tasks. We should begin with the cases in which we know a change is possible and should study the allotropic forms of the elements. At present our knowledge of these is disgracefully incomplete. We know a little about sulphur, phosphorus, carbon, selenium and tin; but even for these few elements our knowledge is incomplete and it is especially unsatisfactory in matters bearing on the rate of change. In most cases the change from one allotropic form to the more stable one is fairly slow. It is not even easy to get large amounts of gray tin. On the other hand, Saunders discovered, quite by ac-

cident, that there were a number of substances, notably quinoline, which convert amorphous selenium into the more stable, black, metallic modification. It is probable that similar results could be obtained with other elements. Kastle has shown that the rate of change of yellow mercuric iodide into the red form varies enormously with the nature of the solvent. The first thing that we need is a systematic study of the allotropic forms of the elements, considering reaction velocity as well as equilibrium. We next take up cases where the change from one form to another can be made increasingly difficult. The three disubstituted benzene compounds, as I have already said, are to be considered as different modifications, only one of which can be stable as solid phase at any given temperature and under atmospheric pressure. According to the text-books *o*-phenol sulphonic acid changes readily into *p*-phenol sulphonic acid on heating. When bromine acts on phenol in the cold, *p*-bromphenol is formed, while *o*-bromphenol is formed when the reaction takes place at 180°. I have not been able to find any record of the *p*-brom compound changing into the *o*-brom compound on heating; but the experiment is worth trying. When we come to the three dibrombenzenes, we have a case where we know that the three forms are identical in composition and where there is certainly some sort of an equilibrium at the time of formation because the relative amounts of the modifications can be changed by varying the conditions of preparation. In spite of all this we know no way of converting two of these compounds directly into the third. We could undoubtedly do it if we could raise the temperature high enough, just as we could also convert the elements. It is as yet impossible to attain the temperature at which the elements change rapidly, while secondary reactions interfere in the case of the organic compounds. So long as we can not change the two less stable forms of any disubstituted benzene compound into the most stable form, there is no reason why we should expect to succeed in what may, perhaps, be the impossible task of simplifying the elements.

Summing up, the future developments in physical chemistry will comprise a theory of concentrated solutions, further applications of the phase rule and of the theorem of Le Chatelier, a systematic study of organic chemistry, and a theory of catalysis.

WILDER D. BANCROFT.

REPORTS OF COMMITTEES.

THE following reports of committees were presented to the council. They were accepted and ordered printed:

On the International Congress of Americanists.

The International Congress of Americanists held its fourteenth biennial meeting in Stuttgart, Germany, August 18–23, 1904. On June 1, 1904, I received a communication from you announcing my appointment as the representative of the American Association for the Advancement of Science at this meeting. The designation was gladly accepted, as it had already been arranged that I should attend the congress on behalf of the Smithsonian Institution.

I now have the honor to report that the meeting was in every way a most gratifying success and that the representation of the American Association was duly recognized and published in the official bulletins of the congress. The attendance was largely German, but representatives from a dozen other countries were present and took an active part in the proceedings. The papers presented related almost exclusively to American history and anthropology and especially to South American subjects. The Germans as well as the French have given very great attention to investigations on that continent.

The next meeting of the congress is to be held at Quebec in August, 1906.

Very respectfully,

W. H. HOLMES.

On Anthropometry.

The committee beg to report that individually and as a committee they have been carrying on

anthropometric work during the past year. It was not feasible to arrange an anthropometric laboratory last year at St. Louis, but this year excellent arrangements have been made in connection with the psychological laboratory of the University of Pennsylvania. Measurements of the members of the association are being made by Messrs. V. A. C. Henmon, F. Bruner and G. C. Fracker, with the cooperation of Professor Thorndike, Dr. Woodworth and members of the committee. The chairman of the committee is making an extended study of American men of science; two papers have been published on the subject and there is now in press a 'Biographical Directory of American Men of Science,' containing much material that can be used. We may call special attention to the Anthropometric and Psychometric Laboratory of the Louisiana Purchase Exposition, arranged by Dr. McGee, head of the Department of Anthropology. The laboratory, under the direction of Dr. Woodworth, assisted by Mr. Bruner, made measurements of about 1,000 representatives of different races, especial attention being paid to the native races of the Philippine Islands.

We ask that the committee be continued and that an appropriation of fifty dollars be made for the expenses of an anthropometric laboratory at the next meeting of the association.

J. McKeen Cattell,
Chairman.

On the Atomic Weight of Thorium.

The work on the 'Complexity of Thorium' by Chas. Baskerville and R. O. E. Davis, referred to in our last report, has been repeated, verified and extended by Fritz Zerban. The investigation was prosecuted partly in the laboratory of the University of North Carolina and is continuing in the College of the City of New York. Larger amounts of the pure thorium compounds have been fractioned. Baskerville and Zerban are at present busied with removing entirely from the new thorium the contaminating constituents preliminary to a determination of its physical constants. Coincident with this work they are studying the properties of the novel impurities, which have been designated 'carolinium' and 'berzelium.' The research is being aided by the Carnegie Institution.

Concerning the second problem assigned your committee for supervision, namely, the work of præseodymium, it would make the following report: Baskerville and G. MacNider did not succeed in proving the complexity of that constituent of the old didymium. The methods of attack were: (1) Production of higher oxides by fusion with sodium dioxide; (2) fractional solution of the well known black oxide in hydrochloric acid at variable temperatures; and (3) fractional precipitation of the oxalate at different temperatures —zero, 20°, and 100° C. A Zeiss comparison spectrometer, purchased by a grant from the council, was used for controlling the progress of the work, which will be continued.

We, therefore, beg leave to report progress.

Respectfully submitted,
Chas. Baskerville,
Capt. Sins Honz,
F. P. Venable.

On Cave Fauna.

Owing to the absence of the secretary of your committee in the caves of Cuba during the last meeting of the association, a report on progress was omitted at the St. Louis meeting.

Since the last report the following papers based in part at least on material collected with the grant of three years ago have been published:

1. 'Report on the Fresh-water Fishes at Western Cuba.' *Bull. U. S. Fish Comm.* for 1902, 211–136.

2. 'The Water Supply of Havana, Cuba,' Science, N. S., XVII., 281–282.

3. 'The Eyes of *Typhlops lumbricalis*, a Blind Snake from Cuba.' *Biol. Bull.*, V., 261–270, by Mrs. E. F. Muhse.

4. 'The Ovarian Structures of the Viviparous Blind Fishes *Lucifuga* and *Stygicola*.' *Biol. Bull.*, VI., 31–54, by H. H. Lane.

5. 'The History of the Eye of *Amblyopsis* from the Beginning of its Development to its Integration in Old Age.' Mark Anniversary Volume, 167–204.

6. 'Divergence and Convergence in Fishes.' In the press of the *Biol. Bull.*

Number five is the most important of these and gives a complete account of the eyes of the largest of our blind fishes. Further work on this form should consist in noting the changes of the eyes in individuals reared in the light.

Several papers are in preparation.

Several years ago a Mr. Donaldson died in Scotland, owner of a farm of somewhat over 182 acres of land near Mitchell, Ind. He was apparently without legal heirs. Suit was brought by the state of Indiana to have this farm escheat to the state. The suit was contested by Scottish heirs of Mr. Donaldson, but was won by the state. This farm is in the midst of the cave region of the

Ohio Valley, to which belong Wyandotte and Mammoth caves, and is much more ideally adapted for experimental work with cave animals than either of the larger caves. On it are easily accessible some very large rooms provided with water. On it are the only entrances to an underground stream which I have followed over a mile by actual measurement and from which all of my material of *Amblyopsis* was obtained. Finally on it the stream comes to the surface under conditions that make the farm admirably adapted for surface ponds and pools to rear cave animals in the light.

The American Association at its Washington meeting passed resolutions asking the state of Indiana to set this aside for a state reservation, and part of it for an experimental farm for the investigation of cave animals, etc. In the winter of 1902 the state legislature passed a bill in part as follows:

"The title of all such lands shall be and remain in the state of Indiana, and such lands shall be devoted to educational purposes.

"The control and management of all such lands shall be vested in the trustees of Indiana University and such lands may be used by said trustees for any proper educational purposes.

"Said board of trustees may in its discretion set off any portion of such grounds to the use of the state board of forestry or to that of Purdue University, or any other educational or scientific institution of the state."

In the meanwhile the heirs appealed the suit to the supreme court of Indiana which also ruled in favor of the state in August of 1903. The heirs thereupon asked the same supreme court to grant them a new hearing before itself and there the matter has been suspended for over a year. It seems very probable that this farm will ultimately pass into the possession of the Indiana University and can then be used for experimental work with cave animals.

I have personally received a grant from the Carnegie Institution which enabled me to make further attempts to secure the embryological material of the Cuban blind fishes, without, however, being entirely successful in securing this much-desired series of embryos.

The most notable and systematic piece of cave work so far undertaken is in preparation by my assistant, Mr. A. M. Banta. He is making a physical and biological survey of Mayfield's cave, situated but five miles from my laboratory. He has determined the distribution of animals in the cave, the per cents. of the total cave fauna that is accidental, occasional or permanent. He is working in the interrelation of these forms and determining the modifications of the permanent members of the fauna to adapt them to cave life. This piece of work will form a base line for future work with the fauna of caves, and it is very desirable that Mr. Banta be enabled to make similar studies of a few selected caves in the various cave regions of America.

It is recommended that the committee be continued and that an appropriation of $100 be made to continue the work of the committee.

Respectfully submitted for the committee.

C. H. EIGENMANN, *Secretary*,
THEO. GILL,
S. H. GAGE.

On Indexing Chemical Literature.

The committee on indexing chemical literature, appointed by your body at the Montreal meeting in 1882, respectfully presents to the Chemical Section its twenty-second annual report, covering the eighteen months ending December 1, 1904.

Works Published:

'A Select Bibliography of Chemistry, 1492–1902,' by Henry Carrington Bolton, Second Supplement. Smithsonian Miscellaneous Collections, No. 1440, City of Washington, 1904.

This supplement brings down the literature of chemistry from the close of the year 1897 to the close of the year 1902. The author died while the publication was in press and most of the proof-reading, as well as the preparation of the index, was done by Mr. Axel Moth, of the New York Public Library.

In the *Arbeiten aus dem Kaiserlichen Gesundheitsamt*, volume 21, pages 141 to 155, appears a critical bibliography of sulfur dioxid in wine, by W. Kerp.

Indexes on the literature of gallium and of germanium, by Dr. Philip E. Browning, of New Haven, Conn., have been completed and accepted by the Smithsonian Institution for publication.

An index to the literature of radium and radioactivity has been completed by Dr. Chas. Baskerville and Mr. Geo. F. Kunz, and is expected to appear in a bulletin of the United States Geological Survey, as an appendix to a paper by Mr. Kunz on radium.

An index to the literature of solubilities, 1875–1903, by Mr. Atherton Seidell, of the Bureau of Soils, is now in the hands of the committee.

The index to the literature of glucinum by Professor Chas. E. Parsons, of New Hampshire College, Durham, N. H., has been completed.

As is well known for a number of years such bibliographies as have been recommended by their committee have been accepted by the Smithsonian Institution for publication in its 'Miscellaneous Collections.' It has thus been possible to put into the hands of specialists and others valuable indexes which could not otherwise be rendered accessible.

That it is not deemed possible for the Smithsonian Institution to continue this work appears from the following extracts from correspondence with Mr. S. P. Langley, Secretary of the Institution:

"The institution has found it necessary to discontinue for the present the publication of separate indices to the literature of the various chemical elements.

"The resources of the Smithsonian Institution, as is well known, are limited, and must be distributed over a very considerable variety of interests. When, failing congressional aid, it seemed that the project of the International Catalogue of Scientific Literature could not proceed without the establishment of an American regional bureau, I decided to assume this on the part of the Smithsonian Institution, and the allotment made for this purpose is practically all that can be spared for any current indexing work.

"The various bibliographies to chemical elements and other chemical indexes could not, apparently, have been projected upon a plan that would fall in with this catalogue, since at the time they were begun no one had the catalogue in mind. Accordingly, I find that the earlier ones come down to 1887, 1893, 1896 and 1900, and a more recent one, thorium, down to 1902. This brings up the entire question of retrospective indexing and bibliography previous to the date 1901, designed to cover the period prior to the beginning of the international catalogue. Such a project for all science should, of course, only be taken up after mature deliberation, and could only be carried through by international cooperation. Meanwhile it seems prudent for the institution to await a careful consideration on the part of all interested in the whole subject, chemistry, being of course, but one of the large group of sciences whose workers must be considered. In view of these considerations, the importance of which you will, I am sure, recognize, I am constrained to leave the entire matter in abeyance for the present."

In view of the above it may be questionable whether the work of this committee has not been completed as far as it is possible to carry out the offices for which it was originally constituted. It may, however, be wise to continue it for another year, to await developments.

In conclusion, references should be made to the great loss sustained by the committee, the section and the association, in the death on November 19, 1903, of Dr. Henry Carrington Bolton, who from the first appointment of this committee has been its chairman. The work of Dr. Bolton in the field of chemical and alchemical bibliography needs no encomium; it is invaluable to all workers in these fields.

JAS. LEWIS HOWE, *Chairman*,
F. W. CLARKE,
H. W. WILEY.

On Electrochemistry.

A pure iridium electrode was purchased, and some rhodium powder. It was deemed advisable to precede the electrochemical portion of the investigation by a study of the chemical phenomena caused by these metals when no current passed. With this in view experiments have been made on the action of these metals on formic acid. These have confirmed the qualitative results of Deville and Debray, that the decomposition products are essentially carbon dioxid and hydrogen under these circumstances and not carbon monoxide and water. The reaction starts at a higher temperature than one would have supposed from the statement of Deville and Debray as to 'gentle heating.' The rate of decomposition of liquid formic acid is constant when the decomposition products are allowed to pass off, but there is need of the further study of the behavior of the acid in a closed space. This will be taken up next, and after that the electrolysis. The effect of the iridium on the chemical and electrochemical equilibrium between chlorine and water will also be studied. For this work your committee asks for a grant of an additional sixty dollars.

The committee begs leave to report progress.

Respectfully,
WILDER D. BANCROFT,
EDGAR F. SMITH.

On Grants.

The committee on grants recommended that the following grants be made for the year 1905:

To the Committee on Anthropometry, $50.

To the Committee on Electrochemistry, $60.

To the Committee on Cave Fauna, $100.

To the Concilium Bibliographicum, $100.

To W. H. Dall, to assist in republishing a rare work on mollusks, the amount to be repaid in the printed volumes, $50.

L. O. HOWARD,
Chairman.

On the Walter Reed Memorial.

At a meeting of the association held in Washington, a committee was appointed, of which I was made chairman, to take such measure as might be found wise for securing a permanent memorial of Major Walter Reed, U. S. A., in recognition of his important services to humanity. Acting under this authority, it was at length found expedient, after several preliminary meetings, to form an incorporation in the city of Washington to hold such funds as might be contributed. This incorporation is now endeavoring to raise the sum of $25,000, of which the income may be paid to Mrs. Reed and the principal may be devoted to a permanent memorial of Dr. Reed. More than $13,000 has been subscribed already, a large part of this amount coming from the medical profession. This is all in addition to the action of Congress, which has given, on the representations of your committee, an unusual pension to Mrs. Reed.

The effort is now making to secure the additional sum of $12,000, and the cooperation of all members of the American Association for the Advancement of Science is urgently desired.

Yours respectfully,
DANIEL E. GILMAN,
Chairman.

On the Relations of the Association to the Journal 'Science.'

We beg to report that the arrangement by which SCIENCE publishes the official notices and proceedings of the association and is sent free of charge to the members in regular standing on payment of two dollars for each appears to give satisfaction. We recommend that the contract with The Macmillan Company be renewed for the year 1905.

SIMON NEWCOMB, *Chairman,*
CARROLL D. WRIGHT,
L. O. HOWARD,
R. S. WOODWARD,
J. McK. CATTELL,
G. K. GILBERT.

The following members of the association were elected fellows:

Section A:

Hayes, Ellen, Wellesley, Mass.
Milham, Willis I., Williamstown, Mass.
Quinn, John Jones, Warren, Pa.

Section B:

Davis, Bergen, New York City.
Lewis, E. Percival, University of California.
Pegram, George Braxton, Columbia Univ., New York City.

Section C:

Dorr, Allen Wade, Washington, D. C.
Martin, F. W., College Park, Lynchburg, Va.
Schober, Wm. B., Lehigh University, South Bethlehem, Pa.

Section D:

Bissell, Geo. W., Ames, Iowa.
Blanchard, A. H., Providence, R. I.
Greene, Arthur Maurice, Jr., Columbia, Mo.
Loewenstein, L. E., South Bethlehem, Pa.
McCaustland, E. J., Ithaca, N. Y.
Wood, Arthur J., State College, Pa.

Section E:

Aguilera, Jose G., Mexico, Mex.
Bawell, Joseph, 105 Bishop St., New Haven, Conn.
Bayley, W. S., Waterville, Me.
Berkey, C. P., New York City.
Bien, Julius, 140 Sixth Ave., New York.
Boutwell, John Mason, Washington, D. C.
Bownocker, J. A., Columbus, Ohio.
Brooks, Alfred Hulse, Washington, D. C.
Bryant, Henry G., 2013 Walnut St., Phila.
Buckley, Ernest R., Rolla, Mo.
Campbell, Henry Donald, Lexington, Va.
Campbell, Marius R., Washington, D. C.
Cobb, Collier, Chapel Hill, N. C.
Collier, Arthur James, Washington, D. C.
Cowles, Miss Louise F., South Hadley, Mass.
Curtis, Geo. C., Boston, Mass.
Douglas, James, 99 John St., New York City.
Fuller, Myron S., Washington, D. C.
Goode, John Paul, Chicago, Ill.
Gordon, Charles H., Seattle, Washington.
Graham, A. W., New York City.
Grimsley, Geo. Perry.
Hayes, C. Willard, Washington, D. C.
Heilprin, A., Academy Natural Sciences, Phila.
Lyman, Benj. S., Philadelphia.
Merriam, John C., Berkeley, Calif.
Penfield, S. L., Yale University, New Haven, Conn.
Tower, Ralph Winfred, American Museum of Natural History, New York City.

Section F:

Allis, E. P., Menton, France.
Bailey, Vernon, Washington, D. C.
Bawden, H. Heath, Vassar College, Poughkeepsie, N. Y.
Beebe, C. W., New York City.
Birge, E. A., Madison, Wis.
Blake, Joseph A., 601 Madison Ave., New York City.

Brown, Arthur Erwin, Phila.
Curtis, Winterton C., Columbia, Mo.
Dahlgren, Ulric, Princeton, N. J.
Davison, Alvin, Lafayette College, Easton, Pa.
Duerden, J. E., Chapel Hill, N. C.
Evermann, Barton W., Washington, D. C.
Glover, M. Allen, Cambridge, Mass.
Guyer, M. F., Cincinnati, O.
Hall, Robert William, South Bethlehem, Pa.
Herrick, Francis Hobart, Cleveland, O.
Hunter, Walter David, Cosmos Club, Washington, D. C.
Jenkins, O. P., Stanford University.
Jones, Lynds, Oberlin, O.
Knower, H. McE., Baltimore, Md.
Linton, Edwin, Washington, Pa.
McGregor, James Howard, New York City.
Mead, A. D., Providence, R. I.
Nachtrieb, H. F., Minneapolis, Minn.
Neal, H. V., Galesburg, Ill.
Osgood, W. H., Washington, D. C.
Rand, Herbert Wilbur, Cambridge, Mass.
Rankin, Walter M., Princeton University, Princeton, N. J.
Raymond, Pearl, Ann Arbor, Mich.
Rice, E. L., Delaware, O.
Torrey, Harry Beal, Berkeley, Calif.
Weysse, Arthur W., Boston, Mass.
Wilder, Harris Hawthorne, Northampton, Mass.
Zeleny, Charles, Chicago, Ill.

Section G:

Ames, Oakes, North Easton, Mass.
Banker, Howard J., Greencastle, Ind.
Berry, Edward W., Passaic, N. J.
Blodgett, Frederick H., College Park, Md.
Burrill, Thomas J., Urbana, Ill.
Cannon, W. A., Tucson, Ariz.
Coker, Wm. C., Chapel Hill, N. C.
Coulter, S. M., St. Louis, Mo.
Duval, Joseph W., Washington, D. C.
Ferguson, A. McG.
Fitzpatrick, Iowa City, Iowa.
Holferty, George M., St. Louis, Mo.
Jeffrey, E. C., Cambridge, Mass.
Kirkwood, Jos. E., Syracuse, N. Y.
Piper, C. V., Washington, D. C.
Pond, Raymond H., 87 Lake St., Chicago, Ill.
Rose, J. N., Washington, D. C.
Shull, G. H., Cold Spring Harbor.
Spillman, Wm. Jasper, Washington, D. C.
Thornber, J. J., Tucson, Ariz.
Wylie, R. B., Sioux City, Iowa.

Section H:

Bair, Joseph H., Boulder, Colo.
Baird, John Wallace, Baltimore, Md.
Churchill, William, New Haven, Conn.
Dellenbaugh, Century Club, New York City.
Fracker, George Cutler, New York City.
Haines, Thomas Harvey, Columbus, O.
Jones, Adam Leroy, New York City.
Kirkpatrick, E. A., Fitchburg, Mass.
Messenger, James Franklin, Winona, Minn.
Spaulding, Edward G., New York City.
Witmer, Lightner, Philadelphia.
Woodbridge, Frederick J. E., New York City.

Section I:

Burton, Theodore E., Cleveland, O.
Clark, Judson F., Montreal, Canada.
Du Bois, William E. B., Atlanta, Ga.
Edmonds, Richard H., Baltimore, Md.
Foote, Allen Ripley, Home Ins. Bldg., Chicago.
MacVannel, John Angus, New York City.
Stoke, Alfred Holt, Greenville, Miss.
Stokes, Anson Phelps, New York City.

Section K:

Abbott, Alexander C., University of Pennsylvania.
Burton-Opitz, Russell, New York City.
Dexter, E. G., Urbana, Ill.
Flexner, Simon, Rockefeller Institute, New York City.
Lindley, Ernest H., University of Indiana, Bloomington, Ind.
Loeb, Leo, University of Pennsylvania.
Meyer, Adolf, New York City.
Smith, Allen J., University of Pennsylvania.
Yerkes, Robert Mearns, Cambridge, Mass.

SCIENTIFIC BOOKS.

Elements of the Differential and Integral Calculus. By W. A. Granville. Boston, Ginn and Company. Pp. xiv + 463.

A characteristic feature of mathematics in the last half century is the increasing attention paid to the foundations and rigorous development of this science. In analysis this movement began with Gauss, Cauchy and Abel in the early years of the nineteenth century and found its greatest exponent in Weierstrass. The movement thus begun has been continued by such men as Riemann, Dedekind, Hankel, Cantor, Jordan, Dini, Stolz, Harnack, Peano and a host of younger men.

As a result of these investigations it was found that much of the reasoning hitherto employed and in current use among mathematicians was either worthless or required to be

modified, restricted or completed. It thus became necessary to rewrite textbooks on analysis or to prepare new ones more in harmony with the new teachings. In this way arose the new edition of Jordan's 'Cours d'Analyse' and Harnack's edition of Serret's 'Calcul,' as well as the new works of Stolz, 'Allgemeine Arithmetik,' and 'Grundzüge'; Tannery, 'Théorie des fonctions d'une variable'; Dini, 'Fondamenti per la teorica delle funzioni di variabili reali.'

In England and America more progressive teachers have felt for some time the need of a modern text-book on the calculus, which is at once rigorous and elementary. The task of writing such a work is not easy. On the one hand, it is necessary to avoid the worthless and even vicious forms of reasoning which mar so many elementary treatises and which are simply intolerable to one educated according to modern standards of rigor. On the other hand, the author must not introduce subtilties of reasoning and logical refinements beyond the needs and comprehension of those who are to use the book.

The volume under review is an attempt to solve this difficult problem. To our mind the efforts of its author have been abundantly crowned with success. In perusing Dr. Granville's book one feels throughout that the author has in mind the requirements of modern rigor. The demonstrations, it is true, often rest on intuition; but this is necessary in a first course, as all will admit. They are, however, usually correct as far as they go, and free from the defects we have mentioned above. We believe the present volume is eminently a safe book to put in the hands of the beginner. He will get no false notions which afterwards will have to be eradicated, with much difficulty; he will, on the other hand, acquire a considerable acquaintance with the principles of the calculus and a good working knowledge of its methods.

We make now a number of criticisms and suggestions.

The definition of limit given in § 29 is not the one given by Cauchy and Weierstrass and now universally accepted. Looked at carefully, we see it supposes that all variables are functions of an auxiliary variable, the time. This leads to unnecessary complications in the definition of the limit of a function in § 32. We believe the strict Weierstrassian definition should be given and used. As an aid to comprehension, the author's notions in these articles might prove useful. In § 34 the notion of a graph is explained; but not with sufficient care, to our mind. How is the reader to know from their graphs that x and $\log x$ are continuous functions? The three properties of the exponential function given in this article result from their arithmetical properties and not from their graph, as the author seems to imply.

The definition of the derivative given in § 41 is not satisfactory; what the author really defines is the differential coefficient at a point. It is their aggregate that forms the derivative.

In § 55 the author has avoided an error which is very prevalent. His passage to the limit is, however, not completely justified. He has yet to show that

$$\lim_{\Delta x=0} \frac{\Delta y}{\Delta v} \cdot \frac{\Delta v}{\Delta x} = \lim_{\Delta v=0} \frac{\Delta y}{\Delta v} \cdot \lim_{\Delta x=0} \frac{\Delta v}{\Delta x}.$$

The demonstration in § 56 should, it seems to us, be replaced by a simpler one. The author obtains the equation

$$1 = \frac{dy}{dx} \cdot \frac{dx}{dy} \qquad (1)$$

and then remarks: if $dx/dy \neq 0$, we have

$$\frac{dy}{dx} = \frac{1}{\frac{dx}{dy}}$$

He should see that there can be no need of making the further assumption, $dx/dy \neq 0$: for if it were, the equation (1) could not exist.

In § 133 the author introduces a double limit without any explanation. As such limits are used in connection with double integrals, § 231, *seq.*, they should be explained with care. The footnote on page 194 is unintelligible to us and certainly will give rise to misapprehension.

The theory of total differentiation does not meet our approval at all. The author has treated the subject from the standpoint that the variables $x_1, x_2, \cdots x_n$ are all functions

of some one variable. Instead of true total differentials, he gets *total derivatives*. The *du* in § 137 are not *total* differentials, but differtials of functions of one variable. In the differentiation of implicit functions the author assumes merely the existence of the partial derivatives. He should assume also their continuity. The form of demonstration is bad, as it requires him to assume (tacitly) the existence of the very thing he is seeking, viz., dy/dx.

In the treatment of envelopes, § 141, the author does not as usual give sufficient conditions for the validity of his reasoning, but contents himself with the vague statement in a footnote that the process is all right 'in all applications made in this book.' This blemish, which a few lines will remedy, should be removed in another edition. The definition of an infinite series given in § 147 is not felicitous. In avoiding the lax definition usually given the author has gone to the opposite extreme. The simplest way seems to be to consider

$$a_1 + a_2 + a_3 + , \ldots \text{ in inf.}$$

as a symbol to which a meaning is attached as to other symbols, as $> < =$, etc. The solution of Ex. 3, § 152, is not quite rigorous, as it postulates the covergence of G. In § 160 *undefined* arithmetical operations are performed on series.

We can not agree with the author that the remainder in Taylor's series for several variables is too complicated to be given. The treatment of maxima and minima can be made much more complete without complications or difficulty. The reasoning given at the bottom of page 248 can be made not only 'plausible,' but entirely conclusive, using no more space that that required by the author.

In the reduction of indefinite integrals the author proves the trivial formulæ

$$\int (du + dv - dw) = \int du + \int dv - \int dw,$$

$$\int a dv = a \int dv,$$

but omits entirely the demonstration relative to the transformation of the variable. This is all the more surprising as this transformation is constantly employed, even in establishing important theorems. Two chapters, XXIX. and XXX., are devoted to definite integrals. In the first we arrive at the notion of a definite integral by means of the notion of area; in the second, by means of the limit of a sum. In our opinion the first treatment is not only superfluous, but should be entirely omitted on several counts.

The relatively few blemishes in this work, the reviewer is glad to state, will be removed in the next edition. JAMES PIERPONT.

YALE UNIVERSITY.

The Study of the Atom, or the Foundations of Chemistry. By F. P. VENABLE. Easton, Pa., The Chemical Publishing Co. Pp. 290.

The history of an important scientific theory is an interesting study, where it is possible, as it often is, to trace the orderly development of that theory from stage to stage. The evolution of the atomic theory is a subject which has claimed the attention of many writers, and the story has been told so often and so well in works on the history of chemistry, that one wonders whether it is not familiar to most chemists. A careful perusal of this book does not disclose any new point of view, or anything new in the method of treatment, though the matter is generally presented in a satisfactory manner, especially Chapter V., which deals with the periodic system. In the last chapter of the book the author considers the most recent hypotheses regarding the constitution of matter by J. J. Thomson, Rutherford and others. The book is generally clear, conservative in tone and, on the whole, well-proportioned, though 75 pages, or one fourth of the contents, seems rather too much to devote to the conception of the atom before the time of Dalton, especially as this material must be taken entirely from secondary sources. The book may be commended as a good summary for students. E. T. ALLEN.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES. SECTION OF GEOLOGY AND MINERALOGY.

THE section was called to order at 8:15 P.M., November 21, 1904, with Vice-president Kemp in the chair and forty persons present.

The first paper of the evening was by Professor J. J. Stevenson, upon 'The Island of Spitzbergen and its Coal,' and was illustrated by lantern slides. In introducing his subject, the speaker described briefly the coast of northern Norway and its geology, and referred in some detail to Bergen, Hammerfest and other cities. Spitzbergen was then taken up, and its coals and their geological relations were passed in review. The coal beds are of Jurassic age, and the coal is peculiar in that it partakes of the characters of the lignites as well as of the true coals.

The second paper on the program was by Professor James F. Kemp, on 'The Titaniferous Magnetite in Wyoming.' On account of the lateness of the hour, the speaker presented his topic only in abstract. The magnetite occurs in two places, fifteen and twenty miles north of Laramie, Wyoming, the former and smaller occurrence being near the Shanton ranch, the latter and larger being on Chugwater Creek. Both are in wall-rock of anorthosite which is practically indistinguishable from anorthosite occurring in the Adirondacks. The ores range from 20 per cent. to 40 per cent. TiO_2. Thin sections show that they contain green spinels, and one slide presents much olivine. They can be most reasonably explained as intrusive dikes. In this view the speaker agreed with Waldemar Lindgren, who has published a brief note regarding them.

JAMES F. KEMP,
Secretary pro tem.

THE section held a special meeting December 2, 1904, with Vice-president Kemp in the chair and two hundred members and visitors in attendance. The meeting was called to order at 8:25 P.M. and the program of the evening was at once taken up. This consisted of a lecture by Professor Albrecht Penck, of the Imperial University at Vienna, who is an honorary member of the academy.

The speaker discussed 'The Glacial Surface Features of the Alps,' and gave a brief summary of some of the results of the twenty years of masterly work which has been done by him and under his direction in the Tyrol. Professor Penck described in popular language the nature of the valleys of the Alps and showed by means of lantern slides and a diagram how the glaciers have widened and deepened portions of their rocky basins and produced lakes.

After a vote of thanks to the distinguished guest of the evening, the section adjourned.

EDMUND OTIS HOVEY,
Secretary.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 592d meeting was held December 10, 1904.

The first paper was read by invitation by Mr. H. H. Kimball, of the Weather Bureau, on 'Variations in Insolation and in the Polarization of Blue Sky-light, during 1903 and 1904.' Observations with an Angström pyrheliometer have been maintained by the Weather Bureau at Washington since April, 1903. Comparison with previous observations at Providence, R. I., and Asheville and Black Mountain, N. C., indicate that the quantity of solar radiation reaching the surface of the earth on clear days during 1903 was considerably less than during 1902 and 1904, the deficiency from April to September being 16 per cent. as compared with 1902, and 9 per cent as compared with 1904.

Observations with a Pickering polarimeter indicate that there have been corresponding fluctuations in the polarization of blue sky-light, the percentage of polarization at a point on a vertical great circle passing through the sun and 90° from it, having averaged 49.6 per cent. from May to October of 1904, as compared with 40.6 per cent. during the same months of 1903.

The work of the astrophysical observatory of the Smithsonian Institution and numerous European observations were quoted, showing similar deficiencies in insolation, in the transmissibility of the atmosphere, and in the polarization of blue sky-light, during 1903.

The subject was considered to be one well worthy of investigation by meteorologists.

Mr. J. F. Hayford, of the Coast and Geodetic Survey, presented some recent results on 'The Computation of Deflections of the Vertical from the Surrounding Topography.'

Deflections may be due to irregularity of density within the earth or to attraction of parts of the earth above the surface of the mean spheroid. By an ingenious method, partly graphical, the author had found it practicable to take account of the influence of all known topographical features on the plumb-line at more than 200 stations; it is usually necessary to consider all the land-masses within 2,500 miles of the station. When these computed deflections from known causes are combined with the deflections found from geodetic measurements, the quantities to be accounted for by irregularities within the earth's surface are usually much greater than had been supposed heretofore.

CHARLES K. WEAD,
Secretary.

MICHIGAN ORNITHOLOGICAL CLUB.

THE Michigan Ornithological Club held its last quarterly meeting for 1904 at the Detroit Museum of Art on December 2. The following program was presented:

P. A. TAVERNER: 'Re Kirtland's Warbler.'

A. W. BLAIN, JR.: 'Some Phases of the Life History of the House Wren.'

J. WILBUR KAY: 'Remarks on the Cowbird.'

DR. P. E. MOODY: 'Nesting of the Blue-gray Gnat-catcher in Wayne and Oakland Counties, Michigan.'

J. CLAIRE WOOD: 'Notes on a Great Horned Owl in Captivity.'

A. B. KLUGH: 'Summer Birds of Puschlinch, Lake Ontario.'

The following were presented by title:

PROFESSOR WALTER B. BARROWS: 'Birds of the Beaver Islands, Michigan.'

DR. MORRIS GIBBS: 'Bird's Nesting.'

WM. H. DUNHAM: 'A Preliminary List of the Birds of Kalkaska County, Michigan.'

PROFESSOR FRANK SMITH: 'An Unusual Flight of Sparrow Hawks in Michigan in 1904.'

CHAS. C. ADAMS: 'A Natural History Expedition to Northern Michigan.'

The next meeting of the society will be held on March 3, 1905. A. W. BLAIN, JR.

DISCUSSION AND CORRESPONDENCE.

INTERESTING AND IMPORTANT FACTS.

IN Powell's 'Truth and Error' a philosophic distinction is made by giving special definitions to the terms *property* and *quality*. A property is an essential characteristic considered in itself; a quality is a characteristic considered in relation to man. Thus the ductility of iron is a property; its utility a quality. The form and coloration of a tree are properties; its beauty or ugliness is a quality. Iron's property of ductility, when thought of in connection with human needs, is a factor of its quality of utility; and the properties of the tree, when viewed from the standpoint of man's esthetic sense, are qualities. This simple distinction is of far-reaching application, because properties are the domain of science and qualities the domain of art. Pure science (with a reservation in respect to anthropology) is not at all concerned with qualities, and when the investigator deals with them he passes into the field of applied science, or the arts. Failure to recognize this distinction leads to much confusion of thought and expression.

One of the milder or less harmful, but at the same time most conspicuous, manifestations of this confusion is connected with the word *interesting*. Not unfrequently an essay ostensibly and mainly scientific will contain the statement that an object, or relation, or other phenomenon is 'interesting,' the context indicating that interest is supposed to inhere in the phenomenon. As a matter of fact, interest is a mental attitude of the observer, and the adjective 'interesting,' though applied to the phenomenon, describes only the observer's relation to it. There are, of course, many legitimate uses of the adjective, and some of these occur in scientific writings. When an author, for example, declares that the insect habits he is about to describe are interesting to students of the psychology of the Bombocoreidæ, it is clear that he does not deceive himself by supposing that he has named a property of the phenomena.

Something similar may be said of *important, valuable,* etc., when employed in scientific description. In common with *novel, pertinent, significant,* and the like, they indicate the relations of phenomena to the condition of human knowledge. Just as each observed fact has at some time, temporarily, the quality of novelty;

so each fact and inference may in some phase of the progress of knowledge serve to explain the previously unexplained, and thus have importance or comparative value. Apart from such temporary and humanistic relations, all facts are equally important or equally unimportant. When, therefore, an author makes the bald statement that a fact is 'important,' he ascribes to it a quality and not a property; and he is self-deceived if he thinks of the importance as an essential characteristic.

It conduces to clear thinking as well as clear writing if one fortifies the use of 'interesting' or 'important' by pointing out the relation which endows the indicated fact with interest or importance. When that has been done the need for the adjective often disappears; and if it can not be done, the adjective is a meaningless platitude.

G. K. GILBERT.

WASHINGTON, D. C.

SPECIALIZATION, IGNORANCE, AND SOME PROPOSED PALLIATIVES.

I BEG leave to use the columns of SCIENCE to express a few ideas which may strike some readers as strangely naïve, but which have been incubating in my brain for a term of years, and must now at length find some mode of deliverance. I speak as one of that large class of unfortunates who aspire to contribute a few small stones to the temple of knowledge, but who are forced to give so much time to purely routine work that little is left for better things. And that precious little remnant of our time—how do we employ it? Largely in misspent energy and unproductive efforts; not in the quest of knowledge, but of the means of acquiring knowledge; not in learning facts, but in learning how to learn! After we have deducted the time spent in purely mechanical operations, in developing our technique and in digesting the ever-growing literature of our particular little fraction of a sub-science, how much remains of those brief moments spared from the struggle for bread? Is it a wonder that 'general culture' suffers, when even our sister sciences are neglected, or that specialization so often results in an intellectual isolation, fatal alike to the scientist and the man? *Platitudes?*—of course they are! Who has not deplored these conditions? But we all resign ourselves to them as inevitable, just as we do to a social order which tolerates boss rule, 'Standard Oil' and the inheritance of poverty or riches. Who has not wished to halt the march of discovery long enough to allow himself to 'catch up'? And, seriously, would it be a misfortune if we should be compelled to pause for a moment in the exploitation of new facts, and properly assimilate the ones we have? But this is not the burden of my modest message.

One can not but marvel at the absence of any adequate bureau of exchange among specialists in different fields of knowledge. We have our societies, it is true, where papers are presented which are oftentimes too technical even for the limited circle of members—all fellow specialists in a single science. We have our journals, congested with contributions, good, bad and indifferent. But which one of us can follow all the technical journals of his own specialty, even though his path be blazed by international bibliographic catalogues? We have our reviews and year-books and Jahresberichten, in which the topics treated are apt to gain in technicality in proportion to the degree of abridgment. Various semi-popular periodicals doubtless do a splendid work in making accessible some of the more general conclusions of science, but their contents are necessarily fragmentary and uncoordinated.

In our higher educational institutions we find specialists engaged in two chief pursuits: giving instruction to students, and conducting research. A third possible function of the faculty seems never to be fully recognized, namely, mutual enlightenment. Why is there often such utter isolation between various departments? Why has there not been established any recognized clearing-house for the exchange of expert knowledge? Much of such exchange doubtless occurs in a desultory and haphazard way, through ordinary social intercourse, so that a man of requisite personal and social gifts may receive and impart much of value. And doubtless various public lecture

courses do something toward meeting this need. But why not organize in every university an inter-departmental congress, in which members of the various departments shall present, in as palatable form as possible, the problems and discoveries of present interest in their respective branches of learning? On the face of things it would seem that such an arrangement would necessarily result in an added stimulus and a broader horizon for each member of the staff, and a greater solidarity for the whole.

An objector will doubtless be prompt to point out that the above plan, though plausible on paper, would, for one reason or another, be quite impossible in practise. I can only reply that no suggestion which offers the least hope of lessening the baneful effects of over-specialization ought lightly to be dismissed. Nor should I be surprised by the quite different criticism that my idea utterly lacks novelty, that it has long since been threshed over by educational experts, perchance received a fair trial somewhere. In reply, I could but cite my own ignorance of these facts as a fine illustration of the very conditions which I have deplored.

But there is another idea which I cherish just as tenderly—one equally chimerical, mayhap. It is nothing more nor less than the establishment of a sort of human encyclopædia as an adjunct to our libraries. How much of our ignorance is due to the inaccessibility of knowledge! How many questions we allow to pass unanswered, rather than grope blindly amongst unfamiliar volumes! The thought lies near to hand that some one could save us that trouble—some one who would not have to grope. But who? The plain man sends a query to his daily paper, and receives an answer which we hope is more trustworthy than the editorial opinions or news items on the same sheet. Or he may have the temerity to write to an expert, who may be good-natured enough to reply. But where in our educational system is the man or body of men whose recognized function it is to answer questions? Teachers we have by the thousand, employed to impart knowledge in accordance with certain more or less stereotyped courses of study, but where are the men whose business it is to tell us just those things for which we happen to be seeking? The scientific departments of our government, it is true, give much expert advice on various matters, in reply to correspondents, and here, indeed, we find our most instructive models. But their scope is obviously limited.

Suppose that one of our great libraries were to employ a staff of consulting experts, men of the rank of college professors, whose duty it should be to furnish definite bits of information in response to legitimate questions, or at least to guide the seeker on his way. The cost of maintaining such a library would doubtless be vastly increased, perhaps doubled—I leave that for the professional librarian to compute. But over against this added cost could be set the untold hours saved to the student or the layman, searching in unfamiliar fields, and the vastly greater facility of the diffusion of knowledge. My suggestion might easily be caricatured into the proposal that the learner should henceforth dispense with books. Quite otherwise, it is my main object to enable him to do more reading and less groping; to peruse pages of text, instead of card catalogues and tables of contents; to economize time, and to minimize the loss of energy through friction.

In the case of a university library, could not such relations be maintained with the faculty as to permit of members of the latter body being called in for expert advice, not sporadically, but as a part of the organic system? This would throw an additional burden upon the teaching staff, which would, of course, need to be increased numerically. But would not such a function compare favorably in usefulness with the teaching of various prescribed subjects to apathetic learners? To the overburdened specialist, such a system would serve the same end as the plan first proposed, giving him more ready access to other fields of thought, and minimizing the evils resulting from the increasing differentiation of knowledge.

But here again I fear that the experts may smile at my modest suggestion, either as being utterly impracticable, or as quite devoid

of novelty or originality. If so, I can but humbly acknowledge my ignorance, adding once more that this unhappy condition merely strengthens my case!

FRANCIS B. SUMNER.

ULTRA-VIOLET LIGHT IN PHOTO-MICROGRAPHY.

TO THE EDITOR OF SCIENCE: Apropos of Dr. Cleveland Abbe's letter in a recent issue of SCIENCE, I would call the attention of your readers to the fact that the developments in the use of utra-violet light in photo-micrography with apparatus designed at Jena is described in some detail in *Engineering* (London), for December 2, 1904, page 760.

CLIFFORD RICHARDSON.

HOW DOES ANOPHELES BITE?

IN a recent number of SCIENCE Professor Washburn, in the course of some remarks on the mosquito exhibit at St. Louis, prepared by me for the New Jersey State Museum, questions the accuracy of a figure of *Anopheles* in the act of biting. I do not understand him to say positively that the figure is inaccurate, only that it had been his belief that the biting position resembled the resting position more nearly. The figure in question, which was a large colored one calculated to attract the attention of the passers-by, was intended to duplicate the picture given by Nuttall and Shipley in their work on *Anopheles,* its structure and habits. It is really a very accurate copy of their plate and the position in my chart is just exactly as published. This is an explanation, not a justification; if the figure is wrong it should not have been put on exhibition in that way; but is it wrong?

When I read Professor Washburn's note I tried to recall my own experience with *Anopheles.* I recall distinctly, watching specimens bite on several occasions, and particularly at Cape May, where *Anopheles crucians* was very plentiful in 1903 and bit freely during the early morning hours. This habit is unusual in the genus and attracted my attention, so that I gave the insects every opportunity to bite; yet, while I can recall distinctly all the surrounding circumstances, I do not recall just what position the insect assumed when biting. I questioned in turn every member of the field and office force, and found that they were equally uncertain in the matter. All of them had been bitten and all of them were able to recall specific occasions where they watched the insect bite, yet none of them would say positively just what the biting position of the insect really was.

During the summer of 1902 Dr. Herbert P. Johnson studied *Anopheles* for me near Newark, N. J., and kept a number of the insects in confinement, allowing them to bite from time to time, and of course watching the operation. I wrote him to the St. Louis University, where he is at present engaged, and received an answer as follows: "While I have not so distinct a mental picture of the operation as I would like to possess I am very confident he [Professor Washburn] is wrong. The biting attitude he mentions would be a most extraordinary one, and for this reason: it is obvious that the mosquito pumping apparatus must penetrate the epidermis before any blood can be drawn and the epidermis is made up of many layers of cells. To thrust its lancets in obliquely is evidently to encounter more resistance, do more work, and with less prospect of success than to thrust vertically through the many layers of cells of the epidermis. If there is an easy way of doing a thing, nature does not ignore it for a more difficult way. The only way in which *Anopheles* could introduce its bill vertically and still keep it in line with its body, would be for the body to assume the vertical position, which I have never seen it do. It is always somewhat oblique."

Mr. Henry L. Viereck, who spent the entire summer at Cape May for me and who especially studied *A. crucians,* writes: "In biting *Anopheles crucians* stand like *A. punctipennis* as shown in Berkeley's figure 17; that is, with the body and beak nearly in a straight line and at an angle somewhat greater than 60° to the surface. The disposition of the legs during the act I can not recall exactly, but I feel quite sure they were very much as in the figure I have referred to."

These communications were hardly satisfactory and we looked up every reference that was available, only to find that no one who

has written upon the subject, whose works we had in hand, spoke definitely on the position of *Anopheles* in actually biting. Nuttall and Shipley describe the bending of the proboscis, and in fact the entire mechanism of biting, and other authors are almost as detailed as they; but to the position, no one seems to refer.

In the hope of getting other information I wrote to Dr. L. O. Howard, and received this reply: "I never saw *Anopheles* bite but once, and that was in the dusk while I was sitting on the platform of a railroad station at Fresno, Cal. My impression was that the beak was not in the plane of the body; but that the head was bent downward. Mr. Pratt, when he was living in Virginia, was frequently bitten by *Anopheles,* and tells me that he has a positive recollection that the head was bent downward and that the hind legs were curved upward."

I wish it distinctly understood that this is not a contribution to knowledge. It is an illustration of how many men may make observations in certain lines and absolutely ignore the most obvious points. It is also intended as a suggestion to those who may have made and recorded direct observations on this point, to publish their experiences.

John B. Smith.

Rutgers College,
New Brunswick, N. J.
December 22, 1904.

SPECIAL ARTICLES.

The Discussion in the British Parliament on the Metric Bill.

There lies before us a reprint from the Parliamentary Debates in the House of Lords on February 23, 1904. The order of the day was the second reading of the bill for the compulsory introduction of metric weights and measures into the United Kingdom of Great Britain and Ireland. We will make some extracts from the discussion which will show the present conditions over there and which will interest us because it is universally admitted that the adoption of the metric system by one branch of the English race will secure its adoption by the other. The reading was moved by Lord Belhaven and Stenton, but the principal advocate of the bill was Lord Kelvin. In order to show how great change had taken place in public opinion on the measure in recent years, as compared with the time not long ago when the chief argument of the opponents of the bill was that public opinion was not yet ripe for it, the noble lord presented petitions from thirty town and city councils, representing a population of over 8,000,000, from fifty chambers of commerce, thirty retail dealers' associations, forty-three trades unions representative of 300,000 workers, sixty teachers' associations, inspectors of weights and measures in eighty districts and a large number of individual signatures, bringing the total number of individuals represented to 333,000. A nearly equal number additional were promised within a week, the Liverpool Chamber of Commerce sending theirs separately by Lord Avebury, together with several chambers of agriculture. The difficulties alleged to be experienced by foreign countries in making the change were declared non-existent. His Majesty's representatives abroad at the time stated the change was made without much difficulty; though some countries were more rapid than others, there never had been any desire to return to the old system, and the adoption of metric weights and measures had assisted the development of trade. Switzerland commenced to use the metric system eighteen months from the passing of the law. There was no great difficulty found there in the towns, but it was some time before it was adopted in remote country places. In Germany it was adopted more quickly than anywhere else. Two years and one month were allowed, and the interval thus granted was sufficient to insure the adoption of the new system in all details; it was an accomplished fact by the day named. There is no desire to go back to the old system, and the change has contributed to a rise of German trade and commerce, foreign trade deriving much benefit. There are some persons who object from a dislike to mental effort, and who prefer to muddle on with the British system described by the prime minister as 'arbitrary, perverse and utterly irrational.' To these I reply that

the metric system is bound to be adopted sooner or later, and that personal inconvenience for a few days should not be allowed to interfere with a measure calculated to promote the trade and prosperity of the country.

We have had nine years of permission to use the metric system without thereby rendering ourselves liable to punishment for a breach of the law, and experience has proved that the change from the system that has been so long in use in this country to a new system can not be made over the whole country voluntarily. *It is a case for compulsion,* and I think the legislature will be thanked by the country for having applied compulsion. In Germany, France and Italy no inconvenience has resulted from the introduction of the metric system, and there has never been such a thing as a complaint. The change in Germany occupied only two years. I have in my hands a statement by Sir Wm. Ramsay, in which he wrote: "I was in Germany during the change there; it gave no trouble whatever and was recognized within a week."

It is interesting to know that the decimal system, worked out by French philosophers, originated in England. In a letter dated November 14, 1783, James Watt laid down a plan which was in all respects the system adopted by the French philosophers seven years later, which the French government suggested to the King of England as a system that might be adopted by international agreement. James Watt's objects were to secure uniformity and to establish a mode of division which should be convenient as long as decimal arithmetic lasted, a thing we may consider as absolutely settled.

I hope this bill will be sent forward with full pressure to the other house, 333 members of which have declared themselves in favor of it and ready to support it.

In introducing the bill, Lord Belhaven and Stenton recalled some of the testimony given in the blue book, known as the 'Report on Weights and Measures,' made to the house of commons July 1, 1895. That report contained three recommendations, viz:

(*a*) That the metrical system of weights and measures be at once legalized for all purposes.

(*b*) That after a lapse of two years the metrical system be rendered compulsory by act of parliament.

(*c*) That the metrical system of weights and measures be taught in all public elementary schools as a necessary and integral part of arithmetic, and that decimals be introduced at an earlier period of the school curriculum than is the case at present.

Of these recommendations the first was complied with by the permissive act of 1897, which made the use of the metric system in trade lawful (it was previously illegal to use it), and the third was adopted under the educational code of 1900. The second is in the bill now before us. An important point in the history of this subject is, that in August, 1902, there was a colonial conference attended by all the premiers of the self-governing colonies, which passed this resolution: "It is advisable to adopt the metric system of weights and measures for use within the empire, and the prime ministers urge the government represented at this conference to give consideration to the question of its early adoption." And since that time the colonies have been pushing the matter with great earnestness.

The saving of time in education by the use of the metric system is not only in the teaching of the tables, but the whole system of compound addition, subtraction, multiplication and division, and the system of computation called 'practise.' Last year inquiries were made of head masters of schools on this subject, and 197 sent replies, of which 161 said the saving would be one year, 30 said it would be two years, and 6 said that it would be three years. The senior mathematical master of Edinburgh high school wrote: "An average scholar would save at least a year and a half, probably two. I conceive it to be not only a saving of time, but an economy of mental effort which is incalculable."

The commercial value of the metric system has been reiterated by British consuls in foreign countries for many years. In the *Board of Trade Journal,* February 15, 1900, the British consul at Amsterdam says: "The iron and steel manufacturers' unions of Germany have adopted a uniform system of

dimensions based on metric weights and measures. The classifications are making more and more progress in Germany, not in the iron trades alone, but in other manufactures. In the future Germany, and the continent generally, will have a constantly increasing advantage over British manufactures in foreign countries, unless the metric system be fully and entirely adopted by Great Britain. I may mention as an undoubted fact that the preference which Germany has obtained here over Great Britain regarding railways, bridges and other railway material is mainly owing to the existence of this metric classification."

Other items in the discussion were that Russia had directed her iron and steel works to alter their rolling machinery so as to produce only rods, rails and sheets on a metric scale, that 45 per cent. of British exports were to non-metric countries and 55 to metric countries (66 per cent. of United States imports are invoiced in metric measures). At present Britain has eighty different denominations represented by 155 different kinds of weights and measures, which by this bill will be reduced to thirty denominations represented by fifty-three different kinds of weights and measures, or only one third the present number.

The bill was read a third time in the house of lords, May 17, and referred to a select committee to arrange the practical details necessary to carry it into effect. It was then passed and sent to the house of commons, and read the first time. This discussion showed that there was a very great popular demand in England for the introduction of the metric system, more than there is in this country at the present time. England is a small country, and the adjacent countries, France, Belgium, Holland and all Scandinavia use the metric system, hence people in general are brought much more in contact with it than in the United States, where we only touch the metric system directly in Mexico, and even this contact is having a decided effect in making the system familiar to our citizens.

The principal arguments now relied on by the opponents of the metric system here are that it has not displaced the old measures in countries where it has been legalized, and that its introduction would be a matter of enormous expense. Any one who has had personal experience in foreign travel, or who will take pains to inquire of any of the thousands of emigrants that come among us, will soon convince himself that the metric system is the principal system in actual use in trade and commerce in European countries.

The very large number of working people who appear in Lord Kelvin's list as advocates of the metric system are drawn to its support not only by the actual contact with metric-using nations, but also by the handicap imposed by the British system on getting a useful practical education. This point is increasing in importance since the complete change of both British and American text-books to the metric system. The absurdity is patent of requiring the workman to use an old system different from that in which all knowledge is gathered by the original workers and communicated to their students, and of which the great mass of operatives are ignorant. The operatives themselves, as soon as they become fully aware of it, demand the possession of this key to knowledge and the higher education.

We have heard a great deal in the last three years about the enormous expense of adopting the metric system. The great majority of people who talk about this expense do not know anything about the actual use of the metric system, and have not brought one scrap of testimony that supports their views from countries that have made the change, while most of those who advocate the system are in the actual use of it as teachers, investigators, etc. The opponents of the system are in the position of a man who condemns a tool without ever having used it. Now Lord Kelvin said in his argument before the lords that "last year inquiries were made of head masters of schools, 197 sent replies, of whom 169 said the saving of time by teaching the metric system would be one year, thirty said it would be two years and six said it would be three years. The senior mathematical master of Edinburgh high school wrote, that in view of the wearing out of teachers and scholars in

obtaining a knowledge of the British system, the adoption of the metric system would result in not only a 'saving of time, but an economy of mental effort which is incalculable.'" Lord Kelvin's argument applies with even more force to the United States. The committee on coinage, weights and measures say in their report No. 1701, April 21, 1902, made to the first session of the fifty-seventh congress: "When we consider there are over 15,000,000 school children in the United States being educated at a public cost of not less than $200,000,000 per annum, the enormity of the waste will be appreciated. In the lifetime of a single generation nearly $1,000,000,000 and 40,000,000 school years are consumed in teaching a system that as a whole does not agree with any other nation in the world, and which does not offer any advantage whatever to compensate for its complexity. Surely the children and teachers of the country are worthy of quite as much consideration as the temporary personal and pecuniary interests of some manufacturers, who have failed to furnish, by either themselves or their representatives, any evidence whatever that the manufactures of Germany or Switzerland have or did suffer any loss whatever by the recent adoption of the metric system in those countries. No one in this country has proposed to affix any penalties legal or otherwise to the use of the customary system, what we do want, and are entitled to work for as citizens, is that the government shall adopt in all its work the metric system, which is already the international system for a majority of the civilized world."

The *American Machinist*, of January 14, sums up the matter by saying what is true: "The testimony of men who have had experience in all parts of the world with both systems in the manufacture of machinery is practically unanimous, that most objections to the metric system are based upon purely *imaginary difficulties*, and that the testimony of men who have not had such experience *does not amount to anything*.

We are constantly asked what advantage will the metric system be to this or that particular business. The whole community is larger than any part of it, and is entitled to first consideration. We have shown above that one of the largest and most important activities in this country, the business of education, will be enormously benefited, and every other trade or business will also be benefited, by the increased effectiveness of mental effort in every direction which is the necessary consequence of substituting a simple and rational system for the complex, irregular and barbarous system now in vogue.

WILLIAM H. SEAMAN.

CURRENT NOTES ON METEOROLOGY.

TEMPERATURES IN THE FREE AIR.

THE valuable data concerning the temperatures in the free air obtained during the daily 'soundings' made at the Prussian Aeronautical Observatory at Berlin, are discussed by J. Homma in the *Meteorologische Zeitschrift* for October, 1904. The observations considered are those of the year 1903, and they are grouped by seasons and by good and bad days, the temperatures being summarized for different altitudes. It is to be noted that the ascents were made at different *morning* hours, between nine and twelve, and, therefore, the mean obtained is not to be regarded as accurately representing the conditions during the twenty-four hours. The vertical temperature gradients for the four seasons show a very slow decrease up to 2,000 meters in winter, and a rapid decrease in spring and summer. The average rate of temperature decrease for the year is about 1.3° per 100 meters near the surface, but decreases aloft, up to about 2,000 meters (0.9°), and then increases with altitude. The mean decrease of temperature for May, June and July between the surface and 500 meters is more rapid than the adiabatic rate of 1.8° in 100 meters.

BAD WEATHER, GOOD ROADS AND FARMERS.

PROFESSOR A. P. BRIGHAM, in the *Bulletin of the American Geographical Society* for December, emphasizes the need of good roads in the United States, and points out how great is the handicap of bad roads to farmers and to railroads. In this connection, the weather is an important factor, for when the roads are

bad, the farmer is greatly restricted in the times when he can go to market. If high prices coincide with a period of wet weather and deep mud, the farmer may lose his opportunity of getting his crops to market. In France, cold or stormy days are often used for hauling to market, but American farmers usually have to use for hauling the days which are the best for work on the farm. Railroad receipts often suffer a serious falling off when the weather is severe, and when the country roads are in such condition that farmers can not haul their produce to the train.

MONTHLY WEATHER REVIEW.

THE *Monthly Weather Review* for September, 1904 (dated November 19), contains the following articles of general interest: H. Elias, 'A New Theory of Fog Formation' (translated from the German); J. H. Spencer, 'Three Notable Meteorological Exhibits at the World's Fair' (the U. S. Weather Bureau, the German and the Philippine Weather Bureau exhibits); and the following notes: 'Meteorology in Roumania'; 'Observations for Twelve Months in Lassa' (data obtained by M. Tysbikov, a Russian, who resided in Lassa from August 15, 1900, to August 22, 1901); 'Observations at the Franco-Scandinavian Station for Aerial Soundings' (from *Comptes Rendus*); 'Wind Velocity and Ocean Waves' (from a recent paper by Cornish).

NO SECULAR CHANGE OF CLIMATE IN TRIPOLI.

VICOMTE DE MATHUSIEULX, in an account of his expedition to Tripoli (*Bull. Amer. Geogr. Soc.*, December, 1904), states it as his opinion that there is no reason for supposing any secular change of climate to have occurred in that region, although others have taken the opposite view. The Latin texts and monuments seem, to this writer, to establish the fact that so far as the atmosphere and soil are concerned, everything is just as it was in antiquity. The present condition of the country is ascribed to the idleness of the Arabs, who have allowed innumerable wells to become choked and the vegetation to perish. "In a country so little favored by nature, the first requisite is a diligent and hard-working population. The Romans took several centuries to make the land productive by damming the ravines and sinking wells in the *wady* beds."

CLIMATIC CHANGE IN THE LAKE CHAD REGION.

THE evidence from the region between the Ubangi River and Lake Chad, studied by M. Aug. Chevalier in 1902–3, is, however, believed to point towards a progressive desiccation there (*La Geographie*, May, 1904). M. Chevalier thinks it probable that a great river once flowed north across the Sahara to the Mediterranean, and that Lake Chad was merely a back water. Vegetable and animal remains indicate an invasion of the Sudan by the Saharan climate, and Neolithic relics indicate the former presence of prosperous communities. The change is not a regularly progressive one, for Lake Chad sometimes spreads beyond its usual bed as a result of several years of heavy rainfall. Since 1897 the waters have continued to fall. After a drought in 1902, Lake Fittri dried up in the following year, and hippopotami which inhabited it went elsewhere.

KITE METEOROLOGY OVER LAKE CONSTANCE.

DR. HERGESELL has contributed to a recent number of the *Beiträge zur Physik der freien Atmosphäre* an account of the observations made by him with kites on the Lake of Constance, the flights being made from a motorboat, loaned by Count Zeppelin, during the years 1900, 1902 and 1903. The observations show that inversions of temperature and of humidity frequently occur in the free air which are not exhibited by the observations made at mountain observatories.

R. DEC. WARD.

THE FIRST OBSERVATIONS WITH 'BALLONS-SONDES' IN AMERICA.

As is known to many readers of SCIENCE, there have been despatched in Europe frequently during the past ten years *ballons-sondes*, or small balloons carrying only instruments that record automatically the temperature and pressure of the air, thus enabling the

temperatures to be determined at the successive heights reached, the place and time at which the balloons fall indicating approximately the direction and velocity of the upper currents. The 'aeronautical concourse' of the St. Louis Exposition afforded an opportunity to undertake these investigations in this country. Accordingly, the work was taken up by Mr. A. Lawrence Rotch, director of the Blue Hill Observatory, in cooperation with Col. J. A. Ockerson, chief of the Department of Liberal Arts at the Exposition, and a series of very satisfactory experiments has just been completed.

The balloons used in the experiments are the closed rubber balloons devised by Dr. Assmann, director of the Prussian Aeronautical Observatory. These balloons are inflated with about 100 cubic feet of hydrogen gas; they expand in rising until they burst, and then the attached parachute moderates the fall. In some cases two balloons, coupled tandem, were employed, and, as only one balloon bursts, the other is borne slowly to the ground and serves to attract attention. The instruments, which were furnished by M. Teisserenc de Bort, of Paris, record the temperature and barometric pressure upon a smoked cylinder, turned by clockwork; and the lightest of them in its basket weighs about one and one half pounds. A notice attached to each requests the finder to pack the instrument carefully in a box and return either to St. Louis or to Blue Hill, with promise of a reward for the service.

Owing to delays in obtaining the gas and apparatus, the experiments were not begun until the middle of September, during which month four ascensions took place. All of the balloons fell within a radius of fifteen miles, about fifty miles east of St. Louis. Twice the height of nine or ten miles was attained where a temperature of 68° F. below zero was recorded. These experiments were conducted by Mr. S. P. Fergusson, of the Blue Hill Observatory staff. Another series of ten ascensions was executed by Mr. H. H. Clayton, meteorologist at the Blue Hill Observatory, during the last part of November and the first days of December, mostly after sunset, in order to avoid the possible effect of insolation. Fortunately, all these balloons were also recovered, though the stronger upper air currents carried them further from St. Louis, three of them traveling more than two hundred miles, and two, at least, with a speed exceeding one hundred miles an hour, the direction of every balloon being toward the easterly semi-circle. Ten of the fourteen ascensions furnished good records, and the reduction of the later ones reveals lower temperatures than in September, for example, 72° below zero at the height of seven and three quarters miles on November 25, and 76° below at six and one quarter miles on the following day.

The fact that all the balloons were recovered indicates the excellent topographical situation of St. Louis for despatching them, and Mr. Rotch expects to make another series of ascensions there this month, in order to obtain the temperatures of the upper air in midwinter.

SCIENTIFIC NOTES AND NEWS.

THE Lavoisier medal of the Paris Academy of Sciences has been awarded to Sir James Dewar.

THE title of Correspondant de l'Ecole d'Anthropologie de Paris has been conferred upon Mr. George Grant MacCurdy of the Yale University Museum.

MR. FREDERIC EMORY, chief of the Bureau of Trade Relations of the Department of State, has presented his resignation to take effect on March 31.

DR. HORACE JAYNE has resigned the directorship of the Wistar Institute of the University of Pennsylvania.

LORD KELVIN has accepted the nomination of the council for the presidency of the London Faraday Society, in succession to Sir Joseph Swan.

PROFESSOR G. SERGI has been made president for the International Congress of Psychology to be held at Rome from April 26 to 30 of the present year.

LIEUTENANT-COLONEL A. KEOGH has been appointed director general of the British Army Medical Service.

At the meeting of the California Academy of Sciences, recently held in San Francisco, Mr. Walter K. Fisher, assistant in the department of zoology, of Stanford University, delivered a lecture entitled 'Bird-life on a Tropical Island of the Pacific.

Dr. Otto Nordenskiold lectured on his Antarctic exploration before the French Geographical Society on December 16.

Plans have been made to erect a memorial to Dr. Franz Riegel, professor of medicine at Giessen, who died last August.

Nature states that it is proposed to establish in the University of Liverpool a memorial to Mr. R. W. H. T. Hudson, late lecturer in mathematics, whose brilliant career was so tragically cut short at the end of last September. The memorial will probably take the form of an annual prize in mathematics, to be awarded for distinction in geometry, the subject in which Mr. Hudson's work chiefly lay.

Dr. Benjamin West Frazier, professor of mineralogy and metallurgy at Lehigh University since 1871, died as the result of a stroke of apoplexy on January 4, at the age of sixty-three years.

Mr. C. C. Barrett, an English entomologist, has died at the age of sixty-eight years.

Sir Lothian Bell, F.R.S., the author of works on metallurgy, died on December 20 at the age of eighty-eight years.

The death is also announced of Professor Hermann Wilfarth, director of the Agricultural Experiment Station at Bernburg; and of M. Paul Tannery, author of works on the history of science.

Foreign exchanges state that the Circolo Matematico di Palermo intends to offer an international prize for geometry at the fourth International Mathematical Congress, which will meet at Rome in 1908. The prize will consist of a small gold medal, to be called the Guiccia medal, after its founder, and of 3,000 francs, and will be given by preference, though not necessarily, to an essay which advances the knowledge of the theory of algebraical curves of space. The treatises may be written in Italian, French, German or English, and must be sent to the president of the Circolo Matematico before July 1, 1907.

The New York *Evening Post* states that in pursuance of the written agreement between Harvard and New York Universities, to carry on for ten years a biological station in the Bermuda Islands, a supervising committee has been completed by the acceptance of a third member of the committee, Hugo Baring, who was nominated by the Royal Society of London, which is a contributor to the enterprise through the Bermuda government. Harvard University is represented on the committee by Hon. Charles S. Fairchild, ex-secretary of the United States Treasury, and New York University by Mr. William M. Kingsley, the treasurer of the university.

Mr. Andrew Carnegie has given $263,000 to the Maryland Institute School of Art and Design, thus doubling the assets of the institution. A new building will be erected to replace the one destroyed in the Baltimore fire. It is also stated that Mr. Carnegie has intimated to officials of the Franklin Institute, of Philadelphia, that if they can secure the Franklin fund amounting to about $155,000, in the hands of the Board of City Trusts, he will add an equal sum to the amount. The Franklin fund, £1,000, was left to the City of Philadelphia by Dr. Benjamin Franklin in 1790, to be used in making loans to young married artificers under certain conditions. No loans under the conditions have been made for years. The money will be used for the erection of a new building.

The Mexican Department of Agriculture is planning a series of meteorological stations to be conected by telegraph with the meteorological observatory in Mexico City.

Professor Boyce, of Liverpool University, has proposed to the Liverpool Chamber of Commerce a scheme for the establishment of a commercial museum and bureau of scientific information.

The new tuberculosis building at the Johns Hopkins Hospital, adjoining the general dispensary, will be formally opened about January 15. It is the gift of Mr. Henry Phipps, of Pittsburg, who gave $20,000 last winter,

through Dr. Osler, for a separate dispensary for tuberculous patients.

KARLSRUHE has followed the example of Charlottenburg in establishing a tuberculosis museum. Arrangements are being made by which parties of working people will be enabled to visit the museum from all parts of the country.

WE learn from *The British Medical Journal* that a private citizen has placed in the hands of the government of the Grand Duchy of Baden a sum of $60,000 towards the foundation at Heidelberg of an institute for the study of cancer. The government has given a site for the purpose in the immediate neighborhood of the University Hospital, and has promised a grant for the maintenance of the institute.

THE completed object-glass of eighteen inches clear aperture for the new observatory was formally delivered to the trustees of Amherst College on December 31 by Mr. C. A. R. Lundin, the maker and optical expert, representing the firm of Alvan Clark and Sons. The objective was brought to Amherst by Professor Todd and deposited in the college vault for safe keeping till the mounting is ready to be erected in the spring. The flint and crown disks were made by Mantois, of Paris, and were pronounced by Alvan Clark the finest pair of disks ever received in his shops. The optical work upon them, figuring, correcting and polishing, during the past two years, fully maintains the highest standard of excellence set by this firm in the forty-inch Yerkes telescope, the thirty-six-inch Lick telescope, the thirty-inch Russian object-glass, the twenty-six-inch at Princeton, and numerous others.

A PACIFIC Coast Biological Society was established at a meeting held in San Francisco on December 10. Its membership includes those who are carrying on research in zoology, paleontology, anatomy, physiology, psychology and botany. Meetings will be held four times a year. At the first meeting Dr. Jacques Loeb gave an address on heliotropism in animals. Professor H. Heath, of the Stanford University department of zoology, was elected president, and Professor W. J. V. Osterhout, of the department of botany at the University of California, secretary-treasurer.

A MATHEMATICAL section of the California Teachers' Association was organized on December 26, 1904, at San Jose. Professor G. A. Miller, Stanford University, was elected president, and Mr. J. F. Smith, Campbell High School, secretary. The main object of the association is to arouse more interest in mathematical pedagogy by means of separate meetings for the discussion of recent mathematical movements.

THE American Breeders' Association will hold its annual meeting at Champaign, Illinois on February 1, 2 and 3. Special sessions will be devoted to the following subjects: specific methods of breeding corn, wheat, apples and other plants; methods of improving short horns, dairy cattle and other breeds of live stock, breeding disease-resisting plants, Mendel's law, in-and-in breeding.

MR. FRANCIS DARWIN has written the following letter to the London *Times:*

In an article on 'Greek at Oxford,' from a correspondent in *The Times* of December 27 occurs the remark 'It will be remembered also that Darwin regretted not having learnt Greek.'

I am at a loss to know on what authority this statement rests. If Darwin had any regrets on the subject of Greek it was when he found that in the two years intervening between leaving school and going up to Cambridge he had almost forgotten his classics, and had to begin again an uncongenial task in order to get a degree.

Darwin says of his education at Shrewsbury School: "Nothing could have been worse for the development of my mind than Dr. Butler's school, as it was strictly classical, nothing else being taught, except a little ancient geography and history" ('Life and Letters,' I., 31). He was, in fact, a victim of that 'premature specialization' which is generally referred to in a somewhat one-sided spirit, and from which the public schoolboy is not yet freed.

If the name of Charles Darwin is to be brought into this controversy it must not be used for compulsory Greek, but against it. In 1867 he wrote to Farrar, 'I am one of the root and branch men, and would leave classics to be learnt by those alone who have sufficient zeal and the high taste requisite for their appreciation' ('More Letters of Charles Darwin,' II., 441).

The Biological Society of Washington offers for sale to the highest bidder, prior to January 15, 1905, its entire accumulation of exchange publications, consisting of about 1,500 serials or parts of serials, pamphlets and volumes on all branches of natural history. An opening bid of $25 is already in hand. The collection will be sold as a whole, and bids for parts can not be accepted. For information apply to the secretary, Wilfred H. Osgood, Department of Agriculture, Washington, D. C.

Dr. Henry Fairfield Osborn, Da Costa professor of zoology in Columbia University and curator in the American Museum of Natural History, will deliver a series of lectures in February, at the Museum of Natural History, on 'The Evolution of the Horse.' The lectures will be given under the auspices of Columbia University in cooperation with the museum, on the first three Mondays and the first three Wednesdays of the month. The subjects are: 'The Horse as an Animal Mechanism,' 'The Horse in Relation of the Idea of Evolution,' 'The Fossil History of the Horse,' especially in North America; 'The Fossil History of the Horse Continued,' 'Existing Races of Horses, Asses and Zebras,' 'Probable Origin of the Domesticated Breeds of Horses.'

A colonial exposition will be held in Marseilles in 1906, for which preparations are being made with great energy. A site has been selected, and 1,500,000 francs has been voted by the department, city and chamber of commerce. The various French colonies have already appropriated 5,000,000 francs to meet their expenses in this exposition, and further financial aid is expected.

Professor Boyce, F.R.S., Dr. Arthur Evans and Dr. H. T. Clarke, who comprise the 13th expedition despatched by the Liverpool School of Tropical Medicine to West Africa for purposes of scientific research, sailed from the Mersey, on December 14. We learn from the London *Times* that on the day before they were entertained to a farewell luncheon by Sir Alfred L. Jones, and a numerous company of Liverpool commercial men were invited to meet them. Sir Alfred Jones in proposing 'Success to the Expedition,' said the Liverpool School had accomplished invaluable work in the direction of making the West Coast more healthful and habitable to white men and natives alike. Mr. James Boyle (American Consul) pointed out that Liverpool was rapidly forging to the front as a scientific center, and both London and Edinburgh would soon have to look to their laurels. Mr. Walter Long, M.P., said that Liverpool and the nation at large owed an incalculable debt to Professor Boyce. In the history of British colonization and territorial acquisition the most painful page was that in which one read of the death, not of men who had fallen sword in hand fighting under the flag of their country, but of those countless thousands whose lives had been sacrificed, as they now believed quite unnecessarily, to a deadly and unseen enemy, which had wrought such terrible ravages in the past, and which they had every reason to believe could now be vanquished. This was a great work, and must result in conquests as great and lasting as any that science had yet achieved. Professor Boyce, in responding, said that the feasibility of Ross's views on this subject had now been proved up to the hilt.

UNIVERSITY AND EDUCATIONAL NEWS.

By the will of the late E. W. Codman of Boston and Nahant, Mass., an estate which it is said, may reach $1,000,000 will be equally divided between Harvard University and the Massachusetts General Hospital.

It is said that the classes graduating from Princeton University from 1881 to 1902 have each pledged $10,000 for the erection of a new dormitory.

The board of trustees of the University of North Carolina has authorized the organization of a School of Applied Science, consisting of the departments of mathematics and engineering, physics and electrical engineering, chemistry, geology and mining. Professor Gore is dean.

Dr. H. Meidinger, professor of applied physics at the technical institute at Karlsruhe, has retired from active service at the age of seventy-three years.